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THE

TRANSATLANTIC LONGITUDE,

AS DETERMINED BY THE

COAST SURVEY EXPEDITION OF 1866.

A REPORT

TO THE SUPERINTENDENT OF THE

U. S. COAST SURVEY.

BY

BENJAMIN APTHORP GOULD,

LATE ASSISTANT.

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THE principal results contained in the following report were communicated to the National Academy of Sciences, and the report in full was afterwards presented for publication to the Smithsonian Institution by the author, Dr. B. A. Gould, with the consent of Prof. Benjamin Peirce, Superintendent of the Coast Survey.

JOSEPH HENRY,
Secretary S. I.

SMITHSONIAN INSTITUTION.
October, 1869.

PREFACE.

THE main facts here presented, together with the numerical results of the field reductions, were communicated to the National Academy of Sciences at their session of January, 1867.

The definite reductions were made during the spring and summer of 1867, Mr. A. T. Mosman assisting in them until May, when he was ordered elsewhere, and Mr. S. C. Chandler, jr., then taking the principal part in them, until they were essentially completed in the following October.

This report, in its present form, was prepared during the year 1867, except the final chapter upon the velocity of the signals, which has chiefly been written during the present month, since receiving from the Superintendent of the Survey permission to print the report.

B. A. GOULD.

CAMBRIDGE, MASSACHUSETTS,
February, 1869.

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ON THE

TRANSATLANTIC LONGITUDE.

I.

ORIGIN OF THE COAST-SURVEY EXPEDITION.

THE determination of longitudes by means of the electro-magnetic telegraph, was, as is well known, first practised by the U. S. Coast Survey; and the methods by which it attained its full development were here in use for several years before they began to be employed elsewhere. From the year 1849 until the beginning of the late war, early in 1861, they were unremittingly prosecuted. At that time, 24 independent determinations had been made, no pains having been spared for the attainment of all possible precision; and the series of telegraphic longitudes extended from the northeastern boundary to New Orleans, covering $2\frac{1}{2}$ hours of longitude and 15° of latitude within our own territory, as well as some portions of the British provinces. Upon the completion of the Pacific Telegraph, arrangements were made[1] for extending the connection to San Francisco; but these were reluctantly deferred in consequence of the condition of the country.

For longitudes reckoned from any trans-Atlantic zero, much coarser methods only have hitherto been available; and the uncertainty of the determinations has been twenty or thirty times greater than that between any of the points which form the series of American determinations, and very much larger than that between any points referred to these fundamental ones, by the geodetic operations of the Survey.

The Atlantic cable promised at last to afford an opportunity of connecting the American with the European longitudes, and thus of reducing the two independent series of determinations into what should practically be but one. The large views of the late honored head of the Coast Survey, Prof. Bache, led him to take immediate steps for the attainment of this end; and upon the first organization of the Atlantic Telegraph Company, to the assistance of which he gave his hearty and effective support, he obtained[2] from the officers of this and of the Newfoundland

[1] Coast Survey Report, 1861, p. 2.
[2] Ibid. 1858, pp. 33, 34, 43; 1859, p. 6.

companies their ready promise of all needful facilities for determining the relative longitude of their terminal stations.

Immediately upon the landing of the cable at Trinity Bay, Mr. Hilgard was dispatched to this remote spot, in order to decide from personal inspection whether the communication was sufficiently good to permit of satisfactory longitude-signals, without delay; but his report was necessarily adverse.

Upon the organization of the telegraphic cable-expedition of 1865, Mr. Hilgard, who during Prof. Bache's illness was acting in his behalf, obtained anew from the respective companies permission for the use of the cable, if successfully laid; and the Hon. Secretary of the Treasury authorized the necessary outlays. Mr. L. F. Pourtales repaired to Heart's Content, and there awaited the arrival of the Great Eastern, in order to inform me without delay of the character and availability of the signals, should the cable be successfully laid; but the rupture of the cable in mid-ocean made his expedition unavailing.

The same preliminary steps were again taken in 1866, Mr. G. W. Dean awaiting the arrival of the Great Eastern at Heart's Content. The expedition of this year was happily successful, and Mr. Dean reported by telegraph that the sharpness of the signals was all that could be desired. Measures were at once taken for organizing the parties. Mr. Dean returned only a few hours too late to present his report while we were attending the session of the National Academy of Sciences at Northampton; but he found Mr. Hilgard and myself at the meeting of the American Association in Buffalo, where all the details for the expedition were arranged without delay, and the needful directions for preparation of instruments and observers given by Mr. Hilgard.

The large interval between the meridians of the two extremities of the cable precluded the employment of the method of star-signals, for many reasons. This method requires a more protracted occupation of the cable than it seemed right or reasonable to solicit; the climate of Newfoundland, according to the best information received, is too uncertain and variable to warrant reliance upon the continuance of a clear sky for three hours, while unless the promise should be favorable it would be unwise to employ the cable for transmitting observations from Valencia, which would be useless unless combined with subsequent observations of the same stars from Heart's Content. Moreover, for a longitude so great as that to be measured, the special advantages of the method of star-signals chiefly disappear; the clock-rates becoming matters of serious importance, and entailing errors of the same order of magnitude as those of the absolute time-determination, while the wide separation of the observers precludes that thorough elimination and control of personal equation which is feasible when the longitude-observations are restricted to zenithal stars, and the observers can easily exchange positions and frequently meet at one or the other station.

There was also ground for confidence that the catalogue of standard stars to be employed for determining time was so well freed from systematic errors, that the difference of half a quadrant in the meridians would introduce no error depending on the right-ascensions, no matter at what hour the comparisons might be made—a confidence which the event has fully justified.

II.

PREVIOUS DETERMINATIONS OF THE TRANSATLANTIC LONGITUDE.

The several determinations of longitude between European and American stations, which have hitherto served as the basis for astronomical and nautical computations, may be classified under the three heads—from moon-culminations, from eclipses and occultations, and from chronometers. Most of them have been referred to one or the other of two American points, the College Observatory at Cambridge, and the Naval Observatory at Washington. The former has presented especial conveniences for the chronometric expeditions, both from its close vicinity to the point of landing and shipment in Boston, and from the charge of these expeditions being confided to the director of the observatory, who was specially versed in chronometric matters, and whose office in Boston was connected with the Cambridge clock by a telegraph wire, so that not even the transportation to the observatory was requisite. The latter, as situated at the national capital, and administered by one of the departments of government, has been naturally selected, in most cases during recent years, as the fundamental point for other determinations. As the European point of reference, Greenwich has been employed in all cases.

The telegraphic longitudes of the Coast Survey have, since the first year, been uniformly referred to a third American point, the "Seaton Station" of the Coast Survey in the city of Washington. But the longitudes of New York and Philadelphia, upon which that of Cambridge depends, were referred to the Washington Observatory, which is situated[1] 12ˢ.44 westward from the Seaton Station, by geodetic measurement. The longitude between Cambridge and Washington, as determined by my predecessor, Mr. Walker, in 1848 and 1849,[2] is as follows:—

Cambridge east from Mr. Rutherfurd's Observatory, New York .	$0^h\ 11^m$	$26^s.07$
Mr. Rutherfurd's east from Jersey City Station (geodetic) . .		11.93
Jersey City Station east from Washington	0 12	3.54
Cambridge (dome) east from Washington	0 23	41.54

and this value has since that time been adopted in all computations, and in the standard books of reference. It must be very near the truth; yet it depends in part upon a geodetic measurement across the Hudson River, where no telegraph wire then existed, and was the earliest determination by the new method, before the employment of many refinements and precautions since introduced. Moreover, the portion between Jersey City and Washington was deduced from the simple telegraphic comparison of clocks—a method which repeated experience, as well as theory, shows to be entirely inferior in precision to the Coast Survey method of star-signals. For this reason, I have more than once urged a redetermination of the only weak link in our chain of telegraphic longitudes, by connecting Mr. Rutherfurd's observatory at New York with the Seaton Station, as well as the Washington

[1] Value determined since that given in the Coast Survey Report, 1851, p. 322.

[2] Coast Survey Report, 1848, p. 22; 1849, pp. 19, 20, 31.

Observatory, by the same methods which have been employed for all the other measurements from our boundary to New Orleans.

Using the values above cited, the following are the determinations of the longitude of Washington from Greenwich which have appeared best entitled to confidence in recent years.

I. *From Eclipses and Occultations.*—These furnished the values generally adopted prior to the year 1848, namely, not less than $5^h\ 8^m\ 14^s$. Thus Gilliss in 1846 used[1] $5^h\ 8^m\ 4^s.6$ for the provisional observatory on Capitol Hill which was,[2] geodetically, $10^s.05$ east of the present observatory. And in the volume of observations made in 1845, the first issued by the Washington Observatory, the adopted longitude is given[3] as $5^h\ 8^m\ 14^s.64$.

Peirce's reductions, in 1845, of occultations observed by Bond at Dorchester from 1839 to 1841 gave[4] $5^h\ 8^m\ 13^s.9$; and Walker, from an elaborate discussion of all available observations between 1769 and 1842, inclusive, obtained[5] $5^h\ 3^m\ 14^s.16$, a value subsequently[6] reduced to $13^s.85$ by change in the adopted longitude of Philadelphia, Cambridge, and Washington.

In 1839 Walker had deduced a new value for the moon's horizontal parallax from a discussion[7] of the eclipse of 1836 May 14, according to which the mean value used by Burckhardt, in the lunar tables employed in the computation of the longitude, required an increase of $1''.52$; and he discovered[8] that, although the probable accidental error of his former result for the longitude of Philadelphia was but $\pm\ 0^s.35$, (subject, however, to the influence of any error in the adopted parallax and semidiameter of the moon,) yet the employment of his new value of the horizontal parallax would diminish the longitudes assigned to all the stations of the Coast Survey by about two seconds of time. Prof. Airy, at Greenwich, had, in reducing the Greenwich observations of 1840, already adopted[9] Henderson's determination,[10] according to which Burckhardt's constant required to be increased by its twenty-six hundredth part. So, too, Olufsen, from discussions,[11] in 1837, of Lacaille's meridian altitudes at the Cape of Good Hope, had inferred the need of an increase of this constant by $2''.24$, and Henderson, in the same year, from his own observations with the mural circle at Capetown, deduced[12] $1''.3$ as the requisite increase. All these investigations, though greatly varying among themselves, agreed in the results that Burckhardt's value was decidedly too small, and thus corroborated the change which Walker's computation of the eclipse of 1836 showed to be necessary. Relying on these confirmations, Walker adopted[6] the correction $+\ 1''.5$ to Burckhardt's constant, and found that the trans-Atlantic longitude deduced from eclipses was thus diminished by $2^s.67$ for the whole coast of the United States. The report

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[1] Gilliss, Astr. Obs. p. x.
[2] By Ellicott's original survey of Washington City. See Coast Survey Report, 1846, p. 72.
[3] Wash. Obs., 1845, p. 87.
[4] Coast Survey Report, 1846, p. 71.
[5] Coast Survey Report, 1848, p. 113.
[6] Ibid. 1851, p. 480.
[7] Transactions Amer. Phil. Soc., VI. 383.
[8] Coast Survey Report, p. 115.
[9] Greenwich Observations, 1840, p. xlviii.
[10] Mem. R. Astr. Soc., X. 283.
[11] Astr. Nachr. XIV. 226.
[12] Mem. R. Astr. Soc., X. 284.

of the Astronomer Royal concerning the reductions of the Greenwich Lunar Observations appeared soon after, and indicated[1] that Burckhardt's coefficient required an increase by its twelve-hundredth part, or 2″.85, thus dissipating any yet remaining doubts as to the necessity of a large diminution of all American longitudes counted from a European meridian.

We thus have at present, from observations of eclipses and occultations—

Walker,[2] corrected value from observations before 1843 . .	$5^h\ 8^m\ 11^s.14$
Peirce,[3] from eclipse of 1851, July 28	11.57
Peirce,[4] from emersions of Pleiades, 1839, Sept. 26 . . .	11.45 ± 0.3
Peirce,[5] " " " 1856—1861 . . .	13.13

but neither of the last three determinations is considered by Prof. Peirce as final.

II. *From Moon Culminations:—*

Walker,[6] from Cambridge observations 1843—45 . . .	$5^h\ 8^m\ 10^s.01$
Loomis,[7] " Hudson " 1838—44 . . .	9.3
Gilliss,[8] " Capitol Hill " 1838—42 . . .	10.04
Walker,[6] " Washington " 1845 . . .	9.60
Newcomb,[9] from " " 1846—60 . . .	11.6 ± 0.4
Newcomb,[10] " " " 1862—3 . . .	9.8

Walker considered $9^s.96$ as the most probable value from moon-culminations, and Newcomb assigned $11^s.1$ as that indicated by those observed at the Naval Observatory from 1846 to 1863, inclusive.

III. *From chronometers transported between Boston and Liverpool.*

Indiscriminate mean[11] from 373 chronometers previous to 1849 .	$5^h\ 8^m\ 12^s.46$
Bond's[12] discussion of 175 chronometers, Expedition of 1849 .	11.14
Walker's[6] " " " " " .	12.00
Bond's[13] " " " " " .	12.20 ± 0.20
Bond's[14] " of 52 chronometers, 6 trips, Expedition of 1855	13.43 ± 0.19

All of these values require to be increased by $0^s.06$, to conform to the new telegraphic determination by the Astronomer Royal of the longitude between Liverpool and Greenwich.

The discordance of results which individually would have appeared entitled to full reliance is thus seen to exceed four seconds; the most recent determinations, and those which would be most relied upon, being among the most discordant. No amount of labor, effort, or expense had been spared by the Coast Survey for its chronometric expeditions, inasmuch as the most accurate possible determination of the trans-

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[1] Monthly Notices R. Astr. Soc., VIII. 186; Mem. R. Astr. Soc., XVII. 52.
[2] Coast Survey Report, 1851, p. 480. [3] Ibid. 1861, p. 195.
[4] Ibid. 1861, p. 220. [5] MS. Coast Survey Report. [6] Ibid. 1851, p. 480.
[7] Astr. Journal, I. 67, using telegraphic longitude of Hudson from Washington as given by Walker, Coast Survey Report, 1851, p. 481. See also Trans. Amer. Phil. Soc., X. 10.
[8] Trans. Am. Philos. Soc., X. 123; Wash. Obs. 1862, vii.
[9] Wash. Obs. 1862, lii. [10] Ibid. 1864, p. 46.
[11] Coast Survey Report, 1851, p. 480. [12] Ibid. 1850, pp. 17, 79.
[13] Ibid. 1854, pp. 120, 138, 141. [14] Ibid. 1856, p. 182.

Atlantic longitude was specially required[1] by law; and the thorough accuracy of Prof. Newcomb's investigations is well known to astronomers. Yet the result of the latest chronometric expedition differs from that deduced by Newcomb,—from moon-culminations observed at the Washington Observatory since its reorganization, compared with those observed at Greenwich,—by more than three and a half seconds of time.

The value employed by the Coast Survey from 1852 to 1859 was $5^h\ 8^m 11^s.2$; since 1859 it has been $5^h\ 8^m\ 11^s.8$.

III.

HISTORY OF THE EXPEDITION.

The building erected in Calais, Maine, and occupied as a longitude-station in 1857, was still in existence, though much dilapidated, the stone piers being undisturbed. Mr. George Davidson, Assistant in the Coast Survey, was to take charge of this station, with Mr. S. C. Chandler, Jr., as aid. Mr. Dean was assigned to the station at Heart's Content, with the assistance of Mr. Edward Goodfellow; while I was to occupy the Valencia station, Mr. A. T. Mosman accompanying. Each station required a small transit-instrument, a chronograph, and an astronomical clock.

The most questionable feature of the arrangement was the use of the land line of wire, about 1100 miles long, between Heart's Content and Calais. Hitherto all our telegraphic longitudes have been determined without any use of "repeaters," or double relay-magnets, which have been most carefully avoided as inevitably introducing an additional element of error, or at least of uncertainty, into the result. The armature-times of different electro-magnets, acted on by galvanic currents of different intensities, enter into the result, and only their mean amount is eliminated, while one-half their difference remains inseparably merged with the resultant longitude. Between Calais and Heart's Content there were known to be not only several of these repeaters, but also one or two stations at least where the messages were received and re-sent by hand, without the intervention even of an automatic "repeater." Yet not only our financial resources, but also our available time and our supply of instruments, precluded the occupation of more than three stations at once, and it was reluctantly decided to make use of so many repeaters in this interval as careful investigation should show to be absolutely necessary.

Messrs. Davidson and Dean left Boston for Halifax in the steamer of Sept. 5, to make an examination of the condition of the telegraph line, and a week later Messrs. Goodfellow, Mosman, and myself sailed in the Cunard steamship Asia, bound for Liverpool, *via* Halifax and Queenstown, taking the instruments for Newfoundland and Ireland. But a short time before our departure the welcome tidings had arrived of the recovery, in mid-ocean, of the lost cable of 1865, and of the successful continuation of this second line to Newfoundland.

To the courtesy and interest of the officers of the Cunard Company we were indebted, from the beginning to the end of our expedition, for many favors and

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[1] Coast Survey Report, 1858, p. 32.

much assistance. The cordial and effective aid of Captain J. P. Anderson, of H. B. M. mail steamer Africa, then temporarily in command of the Asia, was of peculiar value, and calls for the sincerest acknowledgments. I may also mention here our obligations to Mr. Grierson, agent of the Cunard steamship at Queenstown, who, both at the debarkation and reshipment of the instruments, assisted us in the most effective manner.

At Halifax the accounts given by Messrs. Davidson and Dean were far from encouraging. Between the terminus of the Atlantic cable and the American frontier there proved to be four "repeaters" and two stations at which messages were rewritten. Repeaters and batteries were at once provided by us for use at these last-named stations, and it was decided that Mr. Davidson should charter a schooner, in which to visit the various points along the coast of Nova Scotia, Cape Breton Island, and Newfoundland, carrying with him the necessary outfit, and giving the requisite instructions to the operators.

This Mr. Davidson successfully accomplished through great energy and personal exertion, while Mr. Chandler, at his direction, refitted the Calais station, and mounted the instruments; the first observations made there being on the 25th October.

Messrs. Dean and Goodfellow reached Heart's Content on the 20th September, and proceeded to the immediate preparation of an astronomical station; but were not favored with the sight of any celestial luminary until the 16th October, on which day they brought the transit and clock into tolerable adjustment, and on the 18th their regular observations commenced.

On the morning of Saturday, September 22, the Asia arrived off Queenstown, where Mr. Mosman landed with the instruments, while I kept on to Liverpool, and thence to London, to confer with the officers of the Company.

The management and control of the cables being with the Anglo-American Telegraph Company, which had conducted the expedition of 1866, and not with the Atlantic Telegraph Company, on whose friendly promises of assistance we had depended, it became necessary to apply anew for permission to use the lines, and for the needful facilities at Valencia. To the cordial friendliness of George Saward, Esq., Secretary of the Atlantic Company, we had already been indebted for many acts of courtesy, and he aided me without delay in the most effective manner.

The use of the cables was at once granted by John C. Deane, Esq., Secretary of the Anglo-American Company, subject, of course, to the condition that the observations and experiments should not interfere with the regular business of the Company; and I was furnished by him with letters to the telegraphic staff at Valencia. From the eminent Electrician to the Company, Latimer Clark, Esq., I received much valuable information and important practical suggestions, as well as full authority for the trial of electro-magnets in connection with the cables, besides the needle-galvanometers in use by the Company.

The Astronomer Royal also gave his ready sympathy to the undertaking. His own plans had been formed, authority obtained, and some of the preparations already commenced, for making a telegraphic longitude-determination between Val-

encia and Newfoundland in June following; but, with extreme kindness, he placed me in possession of all his special information pertaining to the subject, and aided our operations with word and deed. Subsequently,—when, to my own regret as well as to his, it proved necessary to establish our station at the cable terminus, near the western end of the island of Valencia, rather than at either of the two points for which he had already determined the longitude from Greenwich,—he carried out a third determination of longitude for Valencia, by a telegraphic interchange of signals between Greenwich and our station at Foilhommerum Bay.

On the 1st October I met Mr. Mosman at Killarney. According to previous arrangement he had already brought the instruments to that point by rail, and had visited Valencia to examine the ground, and learn what provision would be required for the stone piers of our transit-instrument and clock, and for the materials of our astronomical station. From his report it was manifest that the requisite supplies could be obtained upon the island, or in its immediate vicinity, and early on the morning of October 2 we started westward. The six large boxes of instruments were piled and carefully made fast upon a large "Irish car," the only vehicle upon springs to be found in the town; and the transportation of this huge tower on wheels for 42 miles, to the ferry across the Straits of Valencia, and the deposit of the instruments in a place of shelter, were accomplished without accident before daylight had wholly disappeared.

The longitude-stations occupied by Mr. Airy in the great chronometer expedition of 1844 (Greenw. Obs'ns, 1845), was at *Feagh Main*, an elevated position previously used as a station by the British Trigonometrical Survey; his transit instrument being placed upon the station-point. For the telegraphic determination of 1862, the instrument used in determining time was mounted in the village of Knightstown, at the eastern extremity of the island. The employment of the same station-point, the position of which was well marked, was, of course, highly desirable. Moreover, it was situated at that point of the island which afforded by far the greatest conveniences, and it was close to the hotel. But the electricians of the Company have always been extremely averse to any connection, however brief, between the cable and any land lines, on account of the possibility of injury to the cable by lightning. This fact, to say nothing of others connected with prompt exchange of messages with Newfoundland, and a readiness to avail ourselves of any sudden change of weather at either place, rendered it imperative that our station should be established very near the building of the Telegraph Company at Foilhommerum Bay, 5½ miles west of Knightstown, and remote from any other dwelling-house except the unattractive cabins of the peasantry.

Here, as close to the telegraph house as was consistent with an unobstructed meridian, the astronomical station was established, and a building constructed, 11 feet wide and 23 in length. This was divided by a transverse partition into two apartments, the larger of these serving as an observatory, while the eastern end was used as a dwelling-place. This building was bolted to six heavy stones buried in the earth, and was protected from the southwest gales by the telegraph house, the corner of which was within a very few yards at the nearest point, while rising ground to the northwest guarded us against the winds from that quarter.

In the observing-room were mounted the transit instrument clock and chronograph. It also contained a table for a relay-magnet and Morse register, and a recording table.

For the kind reception which we met at Valencia, I know not how to give an adequate expression of my thanks. A more hearty welcome, a more thorough and delightful hospitality, a more friendly aid, could have been found at no time or place. The inevitable hardships and exposure of our life, at a distance from any permanent habitation other than the over-tenanted house of the Telegraph Company, and under circumstances apparently incompatible with comfort, were thus mitigated and compensated to an incredible degree. To the Knight of Kerry we were indebted not only for a hospitality worthy the traditional reputation of the land, and for which we shall always remain personally grateful, but also for the most practical and efficient aid in furtherance of our operations. All his agents received instructions to assist us by every means in their power; his buildings afforded storage for our instruments at Knightstown; his quarries and stonecutters furnished piers; his factor enabled us to obtain lumber; and his carpenter was detailed for expediting the work upon our building.

The gentlemen of the telegraphic staff received us with a kindliness to which there was no exception, welcoming us to their quarters, and sharing with us their comforts. Of the sixteen electricians and operators in the service of four different companies, there is no one to whom we are not indebted for essential aid in our work, as well as under personal obligations for many acts of kindness. To Messrs. James Graves, superintendent of the station, and Edgar George, second in charge, we owe especial acknowledgments.

The peculiarly unastronomical sky of Valencia delayed adjustments for a while; but one or two glimpses of the sun at noon enabled us to establish our meridian, and, on the 14th October, at 3 A. M., we obtained transits of a few stars. At that time the observers in Newfoundland had seen neither sun, moon, nor stars; and I am inclined to believe that, excepting the short period when sharp frosts prevail there, the climate of Newfoundland is nearly as unfavorable for astronomical purposes as that of Valencia itself. As regards the Valencia climate, I was informed, on our arrival, that it had rained every day, without exception, for eight weeks. During the seven weeks of our sojourn, there were but four days on which no rain fell; and there was but one really clear night during the period while the instruments were in position. The observations were, in general, made during the intervals of showers; and it was an event of frequent occurrence for the observer to be disturbed by a copious fall of rain while actually engaged in noting the transit of a star.

The method of telegraphing through the Atlantic cable is based upon the ingenious device of Prof. Thomson, in applying to a delicate galvanometer the principle of reflection used by Gauss for heavy magnets. A small mirror, to the back of which is attached a permanent magnet, the joint weight of the two being from five to six centigrams, is held, by means of a single fibre above and below, in the centre of a coil of fine wire, which forms part of the galvanic circuit; and its position and sensitiveness are regulated by movable bar-magnets placed in the

immediate vicinity. Upon the mirror is thrown a beam of light through a slit in front of a bright kerosene lamp, and the deflections of the needle are noted by the movements of the reflected beam, which is received upon a strip of white paper. The exquisite delicacy of this galvanometer, as well as the electrical excellence of the telegraph cables, may readily be appreciated after the beautiful experiment in which the electricians at Valencia and Newfoundland conversed with each other on a circuit not far from 700 myriameters (4320 statute miles) in length, formed of the two cables joined at the ends, using a battery composed of a percussion gun-cap, a morsel of zinc, and a drop of acidulated water.

The absence of any means for the automatic registration of signals received, presented, of course, a very serious obstacle in the way of an accurate longitude determination, inasmuch as the loss of time in noting the signals was not only very considerable, but quite uncertain; but the programme of operations which I had prepared before leaving home was based upon the assumption that the use of self-registering electro-magnetic signals would not be acceptable to the Telegraph Company. All objections to these were, however, waived in our favor by Mr. Latimer Clark in the most cordial manner, and considerable time was expended on two evenings in endeavoring to obtain satisfactory signals which should be self-registering. Unfortunately, these efforts were unsuccessful. The cable could not be discharged with sufficient rapidity for the purpose when the charge was sufficiently strong to actuate our most sensitive electro-magnet. A permanent deflection only was observed at Newfoundland, while the Valencia clock was breaking the circuit during an eighth part of every second; nor did any modification in the character of the battery render these interruptions of continuity perceptible at the other extremity of the cable.

I had previously designed availing myself of an ingenious suggestion of Dr. Gibbs, by which the heat from the lamp should be concentrated and reflected, together with the light, by the mirror-galvanometer; being then received on a very delicate thermo-electric pile, which should thus record upon the chronograph the time of the signals. But too little time was available for the purpose, and although Mr. Farmer, whom I had requested to prepare some apparatus based on this principle, made sufficient progress with his experiments to show the practicability of the suggestion, he was obliged to abandon all hopes of constructing any satisfactory instrument in season to be available for our purposes.

Thus it became necessary to fall back upon the original programme which had been prepared before leaving Boston, and furnished to Messrs. Dean and Davidson. This was as follows:—

PROGRAMME FOR TRANSATLANTIC LONGITUDE CAMPAIGN.

This campaign will consist of two parts, "Heart's Content—Calais," and "Valencia—Heart's Content."

Star-signals being impracticable in each case, the only determinations of longitude will be by comparisons of clocks between the stations; consequently no precautions should be omitted which can in any way increase the precision of the clock-corrections and rates. Only stars of the American Ephemeris should be employed; levels should be continually read during the observations; all

circumpolars should be reversed upon; and stars as far north as 80° should be observed by the old method of eye and ear, instead of the chronograph.

Whenever possible, sets of observations should be made at least twice during the night, each set consisting of not less than three circumpolars (not all at the same culmination), and three time-stars north of the equator, together with any southern time-star which may be convenient. A set of observations should always precede, and another set follow, the exchange of signals, when the weather permits.

One or more of these sets should be computed promptly, that observers may constantly be acquainted with the condition of their instruments. The azimuth error should never remain for more than a day larger than $0^s.2$, nor the collimation error larger than $0^s.1$. For the field computations it will suffice to read off a single tally for each star.

The amount of battery-power and condition of the wire is always to be noted when telegraphic signals are exchanged; also any indications of aurora.

Heart's Content—Calais.

So soon as the instruments are in adjustment, the exchange of clock-signals should commence, and it should be continued nightly, whatever the weather, until the operations for trans-Atlantic longitude are completed at Heart's Content. At the time of the exchange, Calais should notify Heart's Content whether it can determine the clock-correction on the same night; and should transmit the correction deduced for the time of the signals sent on the preceding night.

To exchange clock-signals, put the Calais clock into circuit two or three times, for not more than half a minute at each time and at intervals of at least a minute, while the Heart's Content clock is graduating the chronograph. Arrange the time for putting on the Calais clock, so that the record of 0^s shall be included in the series of its signals. It is very desirable that both chronographs should record this comparison, but if this should be found impossible, the Heart's Content chronograph is the proper one to keep the record. If any confusion is likely to arise as to the precise seconds recorded by the Calais clock, this can be readily obviated by making a couple of quick taps immediately after 15^s, 30^s, or 45^s of the clock-time, entering this fact upon the day-book, and communicating it to Heart's Content.

Valencia—Heart's Content.

1. For this determination, three nights' exchanges through each cable will suffice, provided the clock-corrections are well determined at each station, before and after the exchange. Should circumstances be especially favorable on any occasion, there is no reason why work should not be done with both cables on the same night, thus reducing the requisite number of nights to five.

2. The times at which exchanges will be made must necessarily depend upon the convenience of the Telegraph Company; but the hours between 10 P. M. and 6 A. M. are preferable. (All civil times in this programme are understood to be Greenwich mean times.) Whenever exchanges are to be undertaken, Valencia will notify Heart's Content as early as 6 P. M., if practicable, naming the hour when this can be done. Should no such notice be received by midnight, Heart's Content need not feel obliged to attend farther.

3. At the appointed hour, Valencia will telegraph the word *Gould*, as a notice that all is ready; and upon the reception of the word *Dean* in reply, will begin the signals.

4. The exchange of signals will be effected as follows:—

a). Beginning with a positive current, sets of alternate positive and negative signals will be made, each signal consisting of a single tap half a second in length. The first group will consist of four taps, at intervals of five seconds. Then, after a pause of ten seconds, will follow a group of three taps, five seconds apart; and, after a second pause of ten seconds, yet another group of three taps at five-second intervals; these ten taps, in three groups, constituting a "set." The arrangement of the set will then be thus:—

$$P_{5s}\,N_{5s}\,P_{5s}\,N_{10s}\;\;P_{5s}\,N_{5s}\,P_{10s}\;\;N_{5s}\,P_{5s}\,N$$

and each set will occupy one minute.

b). Two such sets, following one another at an interval of ten seconds, will be sent first from

Valencia; then two sets returned from Heart's Content; and this exchange will be made three times, which will suffice for the telegraphic work of the night. The time requisite will therefore be 2m. 10s. for each series of two sets. Three such series being sent from each station, the time actually consumed for the signals will be but 13m.; so that 20m. will probably suffice for the whole operation.

c). Before sending each series of taps, the sender will call attention by a few rapid alternations of positive and negative signals, to be answered in the same way before he begins the series; consequently the order of proceedings will be as follows:—

First Exchange.
- Valencia gives rapid signals, and Heart's Content responds.
- Valencia sends two series of taps, occupying 2m. 10s.
- Heart's Content gives rapid signals, and Valencia responds.
- Heart's Content sends two series of taps, occupying 2m. 10s.

Valencia then proceeds to give the preliminary signals for a second exchange, and in this way the three exchanges are made. If possible, each observer should then state whether the signals have been successfully received.

5. The length of the taps, and of the intervals between them, is a matter of some importance. Hence a mean-time watch or clock should be used, and the same care taken in giving signals as in making observations. Especially should all the taps be of equal length.

The observer of signals should have the break-circuit key of the chronograph in his hand, and record the earliest indication of deflection. Should the deflection ever be in the reverse direction of that indicated by the programme, this fact should be noted.

6. It may conduce to a better determination of the time of transmission if exchanges are made at different hours of the day. One "set" of ten taps as already described, exchanged at the beginning of each third hour, would probably suffice for this purpose, although each alternate hour would be preferable. These experiments should be made on both cables separately, and, if possible, on the circuit formed by connecting the two cables, without any earth-connection to either. The times for these experiments must be left to subsequent arrangement.

If possible, the following experiments for velocity should be made by use of both cables. They are more important than the system of observations at different hours of the day.

I. The two cables being connected at Heart's Content, but without battery there, Valencia first, and then Heart's Content, will send two sets:—
1. With the two ends to earth at Valencia through battery.
2. With the two ends to earth at Valencia, one through battery, the other direct.
3. With the two ends at Valencia to the two poles of battery without earth connection.

II. The same connection with the Heart's Content battery included in the circuit.

III. (Like I., *vice versâ*). The cables being connected at Valencia without battery; Valencia first, and then Heart's Content will send two sets:—
1. With both ends to earth at Heart's Content through battery.
2. With both ends to earth at Heart's Content, one through battery, the other direct.

IV. The same, with the Valencia battery included in the circuit.

7. At the earliest convenient opportunity after an exchange of signals, each observer will communicate to the other his corrected sidereal time, corresponding to the means of the last set of ten taps received, and the last set of ten taps sent.

On the 24th October, longitude-signals were exchanged with Newfoundland for the first time. Between that date and November 20, four more opportunities had been found, and the entire series of experiments for determining the velocity of signals under different circumstances had been satisfactorily tried, as well as some others which I found practicable at Valencia, although not provided for in the programme.

Meanwhile the Astronomer Royal, who had, with his usual kindness, acceded to

my request for a telegraphic connection between our station-point and Greenwich, and assumed all the labor and embarrassment of the necessary arrangements, had carried out the series of exchanges with Foilhommerum, an undertaking attended with no little inconvenience and vexation from the various difficulties attending land lines, especially when a submarine cable of the length of that across the Irish Channel forms a part of the circuit. After many fruitless attempts, clock-signals were exchanged on three nights, upon two of which the time was well determined at both places.

Upon the 20th November, the weather at Heart's Content, as well as at Valencia, was extremely unpromising; no communication had yet been obtained between that station and Calais, and it seemed best, on all accounts, to bring our cable signals also to an end. After visiting Greenwich to offer such aid in the reduction of the longitude-exchanges with that Observatory as might be acceptable to the Astronomer Royal, Mr. Mosman reached home on the 22d December, and I followed four weeks later.

The personal error, with other loss of time in observing signals, has happily proved more constant and more measurable than I had ventured to anticipate. No matter how great the interval, the resultant longitude will only be affected by one-half the difference of the values for the two observers; while the average value for the two observers could be merged with the time of transmission for the signals. It is not the least satisfactory of our results that this interval proved capable of measurement with a degree of accuracy which leaves no ground for apprehension that it has appreciably affected our value for the longitude, and which enables us to infer the velocity of transmission within restricted limits of probable error.

The exchanges between Heart's Content and Calais were far less satisfactory. Notwithstanding the laborious precautions taken by Mr. Davidson, all efforts at direct communication proved unavailing, day after day, and week after week. Mr. Davidson's health became seriously impaired, and Mr. F. W. Perkins was added to the Calais party, joining it on the 12th November. Finally, Mr. Davidson being called to important duties at the Isthmus of Darien, was compelled to leave Calais, and Mr. Charles O. Boutelle, one of the most experienced officers of the Survey, was assigned to the charge of the station. Still, the necessity of an intermediate astronomical station at Port Hood or Aspy Bay seemed inevitable, when suddenly, on the 11th December, only a couple of hours before Mr. Boutelle's arrival, the long-desired communication was found to be established. A sharp frost had thrown the otherwise defective line into a condition of admirable insulation, so that an interchange of clock-signals was effected without difficulty. Comparisons of clock-time at the two stations were also made on the 12th, 14th, and 16th December, though not in a manner wholly satisfactory, since clouds interfered with the attainment of sufficient observations for time. At this juncture Mr. Dean, at Heart's Content, decided to discontinue observations, and dismount his instruments, so that the work was brought to a close, the Newfoundland observers reaching Boston again in the last week of December.

In reducing the observations, I have been aided to some extent by Mr. Mosman, but chiefly by Mr. Chandler, who has for several years rendered efficient and skilful

service in computations of this kind, as well as in numerous other astronomical observations and reductions. To both these gentlemen I desire to make acknowledgment for their valuable services in the office as well as in the field.

The nature of the undertaking had, of course, thus far precluded any determination of personal equation between the observers. This was provided for with as little delay as possible. My plan had contemplated the entire elimination of this disturbing element at Heart's Content, since it would affect the longitudes of Calais and Valencia equally, but with opposite signs. It proved that this precaution had been overlooked, and that the time had been determined by Mr. Dean during the exchanges of signals with Europe, and by Mr. Goodfellow during those with the United States; but, as will be seen, this proved of no practical importance. During a long series of years the personal equation between these two gentlemen, as determined several times annually, was inappreciable; and so, too, it proved in the comparisons made after their return from the present expedition. At the earliest practicable date extensive observations were made for the determination of the personal equations between each pair of observers. The results of these will be given in their place.

It may, perhaps, be well to add a few words concerning the instruments used, which were the regular apparatus of the telegraphic party of the Coast Survey, consisting at each station of a transit-instrument, a chronograph, and a circuit-breaking clock.

The transit-instruments have an aperture of about 7 centimeters, and a focal length of about 116 centimeters. Each is provided with a reversing apparatus attached to an iron stand, and capable of reversing the instrument with ease in about twenty seconds; so that it is not difficult to observe a star, in one position of the axis, within 30 or 35 seconds after observing it in the other. The illuminating lamps are placed on brackets unconnected with the instrument, and as far from it as possible. The reticule carries five "tallies" or sets, of five spider-lines each, at intervals of about $2\frac{1}{2}$ equatorial seconds of time, the several tallies being separated from each other by twice this distance. The tallies are denoted by letters of the alphabet from B to F inclusive, and the individual threads by subjacent numbers, the numeration beginning with the "Lamp End" or end at which the illumination is admitted to the field, so that when this end is west, a star at its upper culmination traverses the threads in the direct order of their numeration from B_1 to F_5. The instruments are provided with diagonal eye-pieces of magnifying power not far from 100, and signal keys are permanently fixed on each side in convenient positions. The chronographs at Valencia and Heart's Content were "Spring Governors" by Messrs. Bond & Son; that at Calais was a "Kerrison's Regulator," with modifications by Mr. Saxton. Upon all of them one pen, which is constantly tracing a line upon a revolving cylinder, records the signals both of the clock and of the observer by offsets from this normal line.

The experience of eighteen years has shown that the greater simplicity of the apparatus, when provided with but a single electro-magnet and recording pen, far overbalances in the longitude work of the survey any inconveniences arising from a possible confusion of the clock-signals with those given by the observer. The off-

sets produced by the former are of practically equal length, this length depending on the adjustment of the armature and strength of the battery; while those produced by the observation-signals are for a practised observer quite near enough to equality to preclude any difficulty in reading off the records, except in very rare instances. For portable instruments there seems to be no room for reasonable doubt as to the superiority of an instrument with a single pen; and for the fixed instruments of an observatory I should personally give this construction a decided preference. All signals are given by the interruption of a closed circuit, so that, when the observing key is properly adjusted, no interval elapses between the first pressure and the transmission of the telegraphic signal; while the moment of release of the armature from the electro-magnet is distinctly recorded. The clocks are all provided, according to Saxton's plan, with delicate platinum tilt-hammers resting on platinum disks, and so adjusted that a small pin fixed in the pendulum-rod at its centre of percussion shall strike the tilt-hammer at the instant when the rod is vertical, and thus lift the hammer from the disk for a very brief period, generally about the one-hundredth part of a second. The galvanic circuit to the chronograph being conducted through this tilt-hammer and disk, the circuit becomes interrupted for a moment at each oscillation of the pendulum.

The advantages of this mode of recording the clock-signals over any in which the galvanic current traverses any portion of the clock itself, or in which the signals are produced according to Saxton's original plan by contact with a globule of mercury, have been sufficiently set forth in previous reports, and require no repetition here.

IV.

OBSERVATIONS AT VALENCIA.

Here the Krille clock and Transit-instrument No. 4 were employed. I had supposed all precautions taken to insure that the instruments should be in good order; but, owing probably in part to the haste with which the expedition was organized in view of the approach of winter, this was not the case, and the want of proper condition of both these instruments, as well as of the minor telegraphic apparatus, much augmented the unavoidably serious difficulties of the enterprise.

Observations were obtained on fifteen nights during our sojourn at Valencia, on no one of which the sky was unclouded. On only two of the five nights on which longitude-signals were exchanged with Newfoundland was it possible to obtain observations after the exchange, and this was possible, too, on only one of the three nights when signals were successfully exchanged with Greenwich. Observations of circumpolar stars for the special purpose of determining the intervals of the transit threads, were out of the question. Indeed there was but one instance when a transit of any star north of 60° declination was observed over all twenty-five threads. In those rare instances when this would have been possible, the stars were needed for determining the error of collimation.

At the close of the series of observations, it was found that 53 complete transits had been observed over all the threads; and since the equatorial intervals of the

same reticule had been very thoroughly and satisfactorily deduced from an ample series of observations in 1860–61 at Pensacola, it appears that little would probably be gained by an attempt to obtain additional data at Valencia. Indeed, after assorting the thread-intervals deduced from the Valencia observations into three classes, the accordance of the mean values for these classes showed a probable error amounting for but few of the threads to so much as $0^s.02$ of a great circle.

The Pensacola values had been deduced from 121 transits of 21 stars,—the average declination of 9 of them being $75\frac{1}{2}°$. The probable error of but few of the intervals was so large as $0^s.005$; and the combination of these values with those derived from the Valencia observations gives all needful accuracy. The Pensacola values were therefore reduced to the focal adjustment of the instrument at Valencia by diminishing each interval by its three-thousandth part, and a triple weight was assigned to the resultant values.

We thus have, for the equatorial intervals of the several threads from the mean of all, the following determinations:—

EQUATORIAL THREAD-INTERVALS OF TRANSIT No. 4.

	Pensacola values		Valencia, 1866.	Adopted value.
	1860–61.	Reduced to Valencia focus.		
B_1	$+34^s.156$	$+34^s.145$	$+34^s.117$	$+34^s.138$
B_2	31.784	31.774	31.834	31.789
B_3	29.255	29.245	29.311	29.261
B_4	26.850	26.841	26.829	26.838
B_5	24.317	24.309	24.289	24.304
C_1	19.450	19.444	19.424	19.439
C_2	17.136	17.130	17.134	17.131
C_3	14.574	14.569	14.570	14.569
C_4	12.204	12.200	12.187	12.197
C_5	9.799	9.796	9.755	9.786
D_1	4.909	4.908	4.904	4.907
D_2	+2.456	+ 2.455	+ 2.462	+ 2.457
D_3	—0.034	— 0.034	— 0.038	— 0.035
D_4	2.372	2.371	2.366	2.370
D_5	4.717	4.716	4.750	4.724
E_1	9.677	9.674	9.693	9.679
E_2	12.220	12.216	12.175	12.206
E_3	14.634	14.629	14.647	14.634
E_4	17.154	17.148	17.166	17.152
E_5	19.467	19.461	19.442	19.456
F_1	24.438	24.430	24.433	24.431
F_2	26.858	26.849	26.837	26.846
F_3	29.382	29.372	29.361	29.369
F_4	31.770	31.760	31.789	31.767
F_5	—34.168	—34.157	—34.119	—34.147

Levelings of the axis were of course made as frequently as possible, and the correction for inequality of the pivots thence deduced is $-0^s.013$, the perforated

end of the axis being the larger. The value resulting from Pensacola levelings was 0ˢ.015; and the mean of these has been applied to all level-readings as correction for inequality of pivots.

On and after November 5, the transit-observations upon which the longitudes depend were made by Mr. Mosman alone. On the 25th and 28th October, they were made by myself; and on the other dates which enter in any way, however implicitly, into the longitude-determinations transits were observed by both of us. This circumstance, undesirable in itself, was, from the necessities of the case, not to be avoided. I have, however, whenever possible, employed Mr. Mosman's observations only for determining the clock-correction, and for those cases where this was not feasible have applied to my own observations the constant correction of —0ˢ.08 for personal equation to reduce them to Mr. Mosman's, as will be explained in a subsequent chapter.

With these few explanations, and the added remark that the observations for time were almost without exception obtained with extreme difficulty in the intervals of clouds and rain in one of the most unfavorable climates of the globe for an astronomer, I give the crude observations, and their reduction for the groups immediately preceding and following each series of longitude-signals, omitting the others generally as needless. The notation and methods of observation and reduction are those prepared by me for the longitude work of the Coast Survey some fifteen years ago, and are described in detail by Mr. Dean in the appendix to the Coast Survey Report for 1856. The conditional equations for clock-correction and azimuth are solved by least squares, after correcting for level-error and clock-rate; the normal equations and resultant values being appended to each group. As already stated, my own observations have been referred to Mr. Mosman in every case, by subtracting 0ˢ.08 from the observed times.

1866, October 25.—G., Obs.

Star.	Lamp.	Threads	M	b_0	R	Bb_0+k
γ Aquilæ . . .	E.	B_1—F_5	19h 40m 3s.67	+0s.128	0m 0s.000	+0s.087
α " . . .	"	B_1—F_5 [F_4 lost]	44 26.42	+0.132	—0 1.338	+0.087
ε Draconis,[1] U.C. .	"	D_4 $E_{1\,2\,5}$	48 11.45	+0.136	+0 33.603	+0.337
" " .	W.	E_5 $F_{1\,4\,5}$	50 5.12	+0.138	—1 19.972	+0.342
τ Aquilæ . . .	"	C_2—E_5	19 57 45.94	+0.143	+0 0.015	+0.092

$T = 19^h.8$. $\theta = -8^s.3$. $\rho = 0^s.00$. $c = -0^s.019$.

Star.	t	a	Cc	ω_0'	Aa	Δt
γ Aquilæ . . .	19h 40m 3s.76	39m 55s.43	—0s.020	—0.s04	+0s.17	—8s.51
α " . . .	44 25.17	44 16.86	—0.020	—0.03	+0.17	8.50
ε Draconis, U. C. .	48 45.44	48 36.77	-----	—0.37	—0.22	8.45
τ Aquilæ . . .	19 57 46.05	57 37.85	—0.020	+0.12	+0.18	—8.36

$$4\,\Delta\theta + 1.159\,a = -0^s.316$$
$$+ 1.159\,\Delta\theta + 2.259\,a = +0.375$$
$$a = +0^s.243,\ \Delta\theta = -0^s.147,\ \Delta t = -8^s.447.$$

1866, October 27.—M., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
θ' Ceti	E.	B_1—F_5	1h 17m 31s.77	—0s.160	0m 0s.000	—0s.089
γ Piscium . . .	"	B_1—F_5 [$E_{1\,2\,4}$ lost]	24 33.53	—0.132	—0 1.945	—0.119
o " . . .	"	B_1—F_5	38 31.67	—0.104	0 0.000	—0.086
β Arietis . . .	"	B_1—F_5	47 27.40	—0.098	0 0.000	—0.106
50 Cassiopeiæ, U.C.	"	E_1—F_5 [$F_{2\,3}$ lost]	51 16.84	—0.091	+0 5.330	—0.314
" " "	W.	D_5—F_2 [E_3 lost]	1 53 14.49	—0.089	—0 53.402	—0.308

$T = 1^h.7$. $\theta = -8^s.5$. $\rho = -0^s.010$. $c = +0^s.170$.

Star.	t	a	Cc	ω_0'	Aa	Δt
θ' Ceti	1h 17m 31s.68	17m 23s.11	+0s.172	+0s.10	+0s.08	—8s.48
γ Piscium . . .	24 31.47	24 22.92	0.175	+0.13	+0.06	8.43
o " . . .	38 31.58	38 23.15	0.172	+0.24	+0.06	8.32
β Arietis . . .	47 27.29	47 18.73	+0.181	+0.12	+0.05	8.43
50 Cassiopeiæ, U.C.	1 52 21.31	52 12.78	- - - -	—0.03	—0.10	—8.43

$$5\,\Delta\theta + 1.675\,a = +0^s.551$$
$$+ 1.675\,\Delta\theta + 3.156\,a = +0.420$$
$$a = +0^s.091,\ \Delta\theta = +0^s.080,\ \Delta t = -8^s.420.$$

[1] Illumination very bad.

1866, October 28.—G., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
α Cygni	E.	B_1—F_5	$20^h\,37^m\,2^s.92$	—$0^s.059$	$0^m\,0^s.000$	—$0^s.102$
μ Aquarii . . .	"	B_1—F_5	45 37.04	—0.063	0 0.000	—0.041
12 Y. Cat. 1879 U.C.	"	D_4—F_5	51 55.63	—0.069	+1 49.390	—0.422
" " "	W.	E_4—F_4 [F_3 lost]	56 5.04	—0.076	—2 21.419	—0.458
σ^3 Urs. Majoris L. C.	"	B_1—E_2 [E_1 lost]	20 59 15.35	—0.079	—0 29.862	+0.132
ζ Cygni . . .	"	B_1—F_5	21 7 25.42	—0.085	0 0.000	—0.101

$T = 21^h$ $\quad \theta = -8^s.7$ $\quad \rho = -0^s.025$ $\quad c = +0^s.124.$

Star.	t	α	Cc	ω_0'	$\Delta\alpha$	Δt
α Cygni	$20^h\,37\,2^s.82$	$36^m\,53^s.91$	+$0^s.177$	—$0^s.04$	$0^s.00$	—$8^s.74$
μ Aquarii . . .	45 37.00	45 28.03	+0.125	—0.15	—0.01	8.84
12 Y. Cat. 1879 U.C.	53 43.88	53 35.11	- - - -	—0.07	+0.04	8.81
σ^3 Urs. Maj. L.C.	20 59 45.62	58 36.48	+0.329	—0.11	—0 03	8.78
ζ Cygni	21 7 25.32	7 16.56	—0.144	—0.20	—0.01	—8.89

$$5\,\Delta\theta + 1.058\,\alpha = -0^s.347.$$
$$+1.058\,\Delta\theta + 13.708\,\alpha = +0.101.$$
$$\alpha = -0^s.013,\ \Delta\theta = -0^s.112,\ \Delta t = -8^s.812.$$

1866, October 30.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
G. γ Cygni . . .	E.	B_1—F_5	$20^h\,52^m\,22^s.13$	—0^s.067	0^m 0^s.000	—0^s.107
" σ^3 Urs. Maj. L.C.	"	B_1—C_5 exc. C_3	57 45.98	—0.067	+0 59.920	+0.117
" " " "	W.	B_1—C_5 exc. C_3	20 59 45.12	—0.067	—0 59.920	+0.117
" ζ Cygni . . .	"	B_1—F_5 [C_4 lost]	21 7 26.54	—0.063	—0 0.577	—0.077
M. 226 Ceph. U.C.	"	C_4—D_2	22 29 40.24	—0.041	+0 29.371	—0.200
" " " "	E.	B_1—C_5	31 37.52	—0.090	—1 27.850	—0.378
" ϵ Cephei U.C. .	"	E_1—F_5	44 16.12	—0.097	+0 52.953	—0.257
" " "	W.	E_1—F_5	46 2 28	—0.099	—0 52.953	—0.262
" α Pegasi . . .	"	B_1—F_5	22 58 17.89	—0.102	0 0.000	—0.093
G. ϵ Piscium . . .	"	B_1—F_5	23 33 16.48	—0.104	0 0.000	—0.081
" Grmbr. 4163 U.C.[1]	W.	B_1—C_5 exc. B_5	47 21.36	—0.107	+1 17.140	—0.403
" " " "	E.	B_1—C_5 exc. B_4	49 53.75	—0.108	—1 16.140	—0.397
" ω Piscium . . .	"	B_1—F_5	23 52 38.63	—0.108	0 0.000	—0.086

$T = 22^h$ $\theta = -9^s.3$ $\rho = -0^s.030$ $c = -0^s.080$.

Star.	t	α	Cc	ω_0'	Aa	Δt
γ Cygni	$20^h\,52^m\,22^s.02$	$52^m\,12^s.94$	—0^s.106	+0^s.08	+0^s.02	—9^s.24
σ^3 Ursæ Majoris L.C.	20 58 45.67	58 36.70	- - - -	+0.30	+0.13	9.13
ζ Cygni	21 7 25.89	7 16.52	+0.092	0.00	+0.03	9.32
226 Cephei U.C. .	22 30 9.35	30 0.06	- - - -	+0.02	—0.09	9.18
ϵ Cephei U.C. .	45 8.94	44 59.55	- - - -	—0.07	—0.03	9.33
α Pegasi	22 58 17.80	58 8.57	+0.083	+0.19	+0.04	9.15
ϵ Piscium . . .	23 33 16.40	33 7.01	+0.080	+0.12	+0.04	9.23
Groombr. 4163 U.C.	48 37.65	48 28.45	- - - -	+0.16	—0.08	9.07
ω Piscium . . .	23 52 38.54	52 29.42	—0.080	+0.15	+0.04	—9.19

$$9\,\Delta\theta + 1.571\,a = +0^s.954.$$
$$+1.571\,\Delta\theta + 11.581\,a = +0.818.$$
$$a = +0^s.058,\ \Delta\theta = +0^s.096,\ \Delta t = -9^s.204.$$

[1] Very faint; observation difficult.

1866, November 3.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
M. ω Aquarii	E.	B_1—5	$20^h\,46^m\,10^s.28$	—$0^s.055$	—$0^m\,29^s.673$	—$0^s.037$
G. γ Cygni	"	B_1—F_5	52 25.44	—0.053	0 0.000	—0.089
G. σ^3 Urs. Majoris L. C.	"	E_2—F_6	20 59 51.41	—0.046	—1 1.386	+0.090

$T = 21^h$ $\theta = -12^s.4$ $\rho = -0^s.015$ $c = +0^s.009$.

Star.	t	a	Cc	ω_0'	Aa	Δt
ω Aquarii	$20^h\,45^m\,40^s.57$	$45^m\,27^s.94$	+$0^s.009$	—$0^s.22$	—$0^s.32$	—$12^s.30$
γ Cygni	52 25.35	52 12.84	+0.012	—0.18	—0.09	—12.41
σ^3 Urs. Majoris L. C. .	20 58 50.11	58 36.93	—0.023	—0.81	—0.82	—12.30

$$3\,\Delta\theta + 3.437\,a = -1^s.132.$$
$$+3.437\,\Delta\theta + 6.102\,a = -2.074.$$
$$a = -0^s.360,\ \Delta\theta + 0^s.035,\ \Delta t = -12^s.365.$$

1866, November 5.—M., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
ζ Pegasi	E.	B_1—F_5	$22^h\,35^m\,3^s.28$	—$0^s.027$	$0^m\,0^s.000$	—$0^s.030$
ι Cephei U. C.	"	E_1—F_5	44 19.29	—0.033	+0 52.952	—0.107
" "	W.	E_1—F_5	46 5.35	—0.036	—0 52.952	—0.114
α Pegasi	"	B_1—F_5	22 58 21.88	—0.048	0 0.000	—0.049
o Cephei U. C.	"	B_1—C_5	23 12 28.87	—0.050	+0 57.061	—0.155
" "	E.	B_1—D_1	14 19.32	—0.083	—0 53.034	—0.238
θ Piscium	"	B_1—F_5	23 21 27.13	—0.096	0 0.200	—0.177

$T = 23^h$ $\theta = -13^s.1$ $\rho = -0^s.015$ $c = +0^s.009$.

Star.	t	a	Cc	ω_0'	Aa	Δt
ζ Pegasi	$22^h\,35^m\,3^s.25$	$34^m\,49^s.79$	+$0^s.009$	—$0^s.36$	—$0^s.39$	—$13^s.07$
ι Cephei U. C.	45 12.21	44 59.32	- - - -	+0.21	+0.32	13.21
α Pegasi	22 58 21.83	58 8.49	—0.009	—0.28	—0.36	13.02
o Cephei U. C.	23 13 25.91	13 13.39	- - - -	+0.58	+0.40	12.92
θ Piscium	23 21 27.05	21 13.56	+0.009	—0.38	—0.41	13.06

$$5\,\Delta\theta + 0.767\,a = -0^s.225.$$
$$+0.767\,\Delta\theta + 2.184\,a = -1.215.$$
$$a = -0^s.571,\ \Delta\theta = +0^s.042,\ \Delta t = -13^s.058.$$

1866, November 6.—M., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
σ^3 Urs. Majoris L.C.	E.	E_1—F_4	20h 59m 32s.45	—0s.020	—0m 42s.798	+0s.056
β Cephei' U.C. . .	"	D_1—F_5 [F_4 lost]	21 26 32.07	—0.053	+0 39.124	—0.187
θ Aquarii[3] . . .	"	B_{12} $C_{2\,4\,5}$	22 10 22.46	—0.053	—0 20.719	—0.036
π "	"	B_1—F_5	18 42.11	—0.061	0 0.000	—0.048
ρ Draconis[3] . . .	"	C_1—$_5$ $E_{1\,2}$	23 17.83	—0.068	+0 31.132	+0.229
ζ Pegasi	"	B_1—D_5	35 17.91	—0.083	—0 14.873	—0.073
α "	"	B_1—F_5	22 58 21.79	—0.072	0 0.000	—0.069
o Cephei[4] U.C. . .	"	E_1—F_5	23 12 29.70	—0.060	+0 57 094	—0.180
" " . .	W.	F_1—$_5$	14 42.73	—0.058	—1 16.158	—0.176
λ Draconis L.C. .	"	B_1—D_5 [B_5 lost]	24 18.09	—0.052	—0 40.896	+0.071
ι Piscium	"	B_1—F_5	33 20.20	—0.051	0 0.000	—0.045
ω " [5]. . . .	"	B_1—E_5 [$E_{1\,2}$ lost]	23 52 33.14	—0.038	+0 9.548	—0.037

$T = 26^h$ $\theta = -13^s.2$ $\rho = -0^s.039$ $c = +0^s.050$.

Star.	t	α	Cc	ω_0'	$A\alpha$	Δt
σ^3 Urs. Majoris L. C. .	20h 58m 49s.71	58m 37s.13	—0s.132			
β Cephei U.C. . . .	21 27 11.01	26 57.47	+0.146	—0s.24	—0s.04	—13s.40
θ Aquarii	22 10 1.71	9 48.54	+0.051	+0.05	+0.04	13.19
π "	18 42.06	18 28.92	+0.050	+0.08	+0.04	13.16
ζ Pegasi	35 2.96	34 49.78	+0.051	+0.05	+0.03	13.18
α "	22 58 21.72	58 8.48	+0.051	+0.01	+0.03	13.22
λ Draconis	23 37 27	23 23 95	+0.147	+0.05	+0.12	13.27
ι Piscium	33 20.16	33 6.94	—0.050	—0.05	+0.04	13.28
ω "	23 52 42.65	52 29.37	—0.050	—0.10	+0.04	—13.33

$$9\,\Delta\theta + 5.303\,a = -0^s.073.$$
$$+5.303\,\Delta\theta + 10.765\,a = +0.325.$$
$$a = +0^s.048,\ \Delta\theta = -0^s.036,\ \Delta t = -13^s.236.$$

1866, November 6.—M., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
ζ Arietis .	W.	B_1—F_5 [$F_{1\,5}$ lost]	3h 7m 26s.87	—0s.015	+0m 3s.061	—0s.024
α Persei .	"	B_1—F_5 [C_5 D_1 E_5 F_2 lost]	3 15 2.96	—0.015	+0 2.495	—0.043

$T = 3^h$ θ = ... ρ = ... $c = +0^s.050$.

Star.	t	α	Cc	ω_0'	$A\alpha$	Δt
ζ Arietis.	3h 7m 29s.91	7m 16s.61	—0s.053	- - - -	+0s.03	—13s.38
α Persei	3 15 5.41	14 52.14	—0.076	- - - -	0.00	—13.35

Assumed $a = +0^s.048$, $\Delta t = -13^s.364$.

[1] Very faint through clouds.
[2] Very faint through clouds.
[3] "Observation doubtful."
[4] Very bad, observation doubtful.
[5] Faint, observation uncertain.

1866, November 8.—M., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
ε Delphini . . .	W.	B_1—F_5	20h 27m 4s.83	+0s.050	0m 0s.000	+0s.028
Groombr. 3241 U.C.	"	C_1—D_3	30 15.47	+0.044	+0 32.676	+0.094
" " "	E.	C_1—$_5$	20 31 36.26	+0.022	—0 47.521	+0.024
ζ Cygni	"	B_1—F_5	21 7 30.70	—0.032	0 0.000	—0.044
α Cephei U.C. . .	"	E_1—F_5	14 52.61	—0.018	+0 46.794	—0.068
" " . .	W.	D_1—F_5	16 22.55	—0.008	—0 43.455	—0.047
β Aquarii . . .	"	B_1—F_5	21 24 46.94	+0.021	0 0.000	+0.001
θ " . . .	"	B_1—F_5	22 10 2.68	—0.012	0 0.000	—0.016
η " . . .	"	B_1—$_5$ $D_{1,2}E_1$—F_1F_4	22 28 43.67	—0.025	+0 1.741	—0.025

$T = 21^h$ $\theta = -14^s.3$ $\rho = -0^s.068$ $c = +0^s.075$.

Star.	t	a	Cc	w_0'	Aa	Δt
ε Delphini	20h 27m 4s.86	26m 50s.80	—0s.051	+0s.15	+0s.14	—14s.29
Groombr. 3241 U.C. .	20 30 48.50	30 33.93	- - - -	—0.31	—0.24	14.37
ζ Cygni	21 7 30.66	7 16.35	+0.057	+0.06	+0.09	14.33
α Cephei U.C.	21 15 39.19	15 24.84	- - - -	—0.04	—0.08	14.26
β Aquarii	21 24 46.94	24 32.80	—0.050	+0.14	+0.17	14.34
θ "	22 10 2.66	9 48.51	—0.051	+0.18	+0.19	14.31
η "	22 28 45.38	28 31.14	—0.050	+0.11	+0.17	—14.36

$$7\,\Delta\theta + 2.134\,a = +0^s.287$$
$$+2.134\,\Delta\theta + 4.161\,a = +0.840$$
$$a = +0^s.214,\ \Delta\theta = -0^s.023,\ \Delta t = -14^s.323.$$

1866, November 9.—M., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
γ Aquilæ	W.	B_3—F_5	19h 40m 12s.43	—0s.016	—0m 1s.445	—0s.021
α "	"	B_1—F_5	44 32.40	—0.015	0 0.000	—0.021
β "	"	B_1—F_5	49 1.61	—0.013	0 0.000	—0.020
τ "	"	B_1—F_5	19 57 53.42	—0.010	0 0.000	—0.017
α^2 Capricorni	"	B_1—F_5	20 10 55.13	+0.004	0 0.000	—0.012
$\varkappa$ Cephei U. C. . . .	"	D_1—F_5	14 41.58	+0.012	—1 6.549	—0.011
π Capricorni	"	C_2—F_5	20 6.34	+0.021	—0 9.201	—0.003
ε Delphini	"	B_1—F_5	27 6.62	+0.021	0 0.000	+0.006
Groombr. 3241 U. C. .	"	B_3—C_5	29 47.39	+0.017	+1 2.357	+0.011
" " " .	E.	B_1—D_3	31 47.02	+0.015	—0 56.685	+0.006
α Cygni.	"	B_1—F_5	37 9.75	—0.008	0 0.000	—0.031
ω Aquarii	"	B_1—F_5	20 45 43.72	—0.011	0 0.000	—0.015

$T = 20^h$ $\theta = -15^s.9$ $\rho = -0^s.090$ $c = +0^s.050$.

Star.	t	α	Cc	ω_0'	Aa	Δt
γ Aquilæ	19h 40m 10s.96	39m 55s.19	—0s.051	+0s.05	+0s.11	—15s.97
α "	44 32.38	44 16.62	—0.051	+0.07	+0.12	15.95
β "	49 1.59	48 45.81	—0.050	+0.05	+0.12	15.97
τ "	19 57 53.40	57 37.62	—0.050	+0.06	+0.12	15.96
α^2 Capricorni	20 10 55.12	10 39.34	—0.051	+0.09	+0.16	15.97
$\varkappa$ Cephei U. C. . . .	13 35 02	13 18.93	—0.237	—0.41	—0.33	15.97
π Capricorni	19 57.14	19 41.27	—0.152	+0.01	+0.17	16.06
ε Delphini	27 6.63	26 50.78	—0.051	+0.04	+0.11	15.97
Groombr. 3241 U. C. .	30 50.05	30 33.85	- - - -	—0.26	—0.19	15.96
α Cygni.	37 9.72	36 53.58	+0.070	—0.12	+0.02	16.04
ω Aquarii	45 43.70	45 27.85	+0.051	+0.16	+0.15	—15.89

$$11\,\Delta\theta + 3.361\,a = -0^s.252.$$
$$+3.361\,\Delta\theta + 10.096\,a = +1.488.$$
$$a = +0^s.171,\ \Delta\theta = -0^s.077,\ \Delta t = -15^s.977.$$

1866, November 9.—M., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
σ^3 Ursæ Majoris L. C. .	E.	$B_1—C_5$	$20^h\,57^m\,54^s.82$	$—0^s.008$	$+0^m\,57.754$	$+0^s.040$
" " " . .	W.	$B_1—D_2$ F_5	20 59 42.25	—0.006	—0 49.772	+0.038
ζ Persei	"	$B_1—C_1$ F_5	3 46 10.41	—0.006	—0 6.259	—0.017
γ Tauri	"	$C_1—D_5$	4 12 23.26	—0.019	+0 7 606	—0.026
ε "	"	$B_1—F_5$	21 8.42	—0.021	0 0.000	—0.028
α "	"	$B_1—F_5$	28 34.62	—0 016	0 0 000	—0 022
α Camelop. U. C. . .	"	$B_2—F_5$	4 41 10.10	—0.015	0 0.000	—0.066

$T = 4^h$ $\theta = —16^s.7$ $\rho = —0^s.090$ $c = +0^s.050$.

Star.	t	α	Cc	ω_0'	$A\alpha$	Δt
σ^3 Ursæ Majoris L. C. .	$20^h\,58^m\,52^s.57$	$58^m\,37^s.35$	- - - -	$+0^s.84$	$+0^s.77$	$—16^s.63$
ζ Persei	3 46 4.13	45 47.72	—0.058	+0.21	+0.14	16.63
γ Tauri	4 12 30.84	12 14.45	—0.052	+0.28	+0.21	16.63
ε "	21 8.39	20 52.01	—0.053	+0.30	+0.20	16.60
α "	28 34.60	28 18.30	—0.052	+0.39	+0.21	16.51
α Camelop. U. C. . . .	4 41 10.03	40 53.25	—0.123	—0.14	+0.21	—16.64

$$6\,\Delta\theta + 3.893\,\alpha = +1^s.870.$$
$$+3.893\,\Delta\theta + 6.863\,\alpha = +2.680.$$
$$\alpha = +0^s.338,\ \Delta\theta = +0^s.092,\ \Delta t = —16^s.608.$$

1866, November 13.—M., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
ι Cephei U. C. . .	E.	$B_1—D_5$	$22^h\,45^m\,52^s.21$	$—0^s.073$	$—0^m\,35^s.320$	$—0^s.201$
α Pegasi	"	$E_{2\,3}$ F_1	22 58 8.53	—0.073	+0 17.650	—0.069
θ Piscium . . .	"	$B_1—F_5$ [D_2 lost]	23 21 31.31	—0.073	0 0.000	—0.060
ω "	"	$B_1—F_5$	23 52 47.23	—0.073	0 0.000	—0.061

$T = 23^h$ $\theta = —17^s.8$ $\rho = —0^s.015$ $c = +0^s.050$.

Star.	t	α	Cc	ω_0'	$A\alpha$	Δt
ι Cephei U. C. . . .	$22^h\,45^m\,16^s.69$	$44^m\,58^s.97$	$+0^s.121$	$+0^s.20$	$+0^s.07$	$—17^s.67$
α Pegasi	22 58 26.11	58 8.39	+0.051	+0.13	—0.07	17.60
θ Piscium	23 21 31.25	21 13.46	+0.050	+0.17	—0.08	17.65
ω "	23 52 47.17	52 29.29	+0.050	—0.02	—0.08	—17.73

$$4\,\Delta\theta + 1.506\,\alpha = +0^s.377$$
$$+1.506\,\Delta\theta + 1.736\,\alpha = +0.006$$
$$\alpha = —0^s.115,\ \Delta\theta = +0^s.137,\ \Delta t = —17^s.663$$

1866, November 13.—M., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
α Ceti	E.	B_1—F_2 [$C_{1\,4\,5}$ D_1 lost]	2h 55m 41s.23	—0s.064	—0m2s.731	—0s.053
α Persei	"	B_1—F_5 [B_5 $C_{1\,2}$ $D_{1\,2}$ do.]	3 15 5 07	—0.064	+0 5.047	—0.118
γ^2 Ursæ Minoris L.C.	"	$D_{3\,4\,5}$ E_4—F_5	3 22 12.26	—0.064	—1 2.569	+0.158
Groombr. 2320 L.C.	"	B_1—C_5	4 5 5.21	+0.003	+1 6.312	+0.296
γ Tauri	W.	B_1—F_5	12 32.30	+0.014	0 0.000	+0.002
ϵ "	"	B_1—F_5	21 9.85	+0.010	0 0.000	—0.001
α "	"	B_1—$_4$ D_1—$_5$ $F_{2\,3}$	28 29.90	—0.007	+0 6.254	—0.016
α Camelop. U. C. .	"	B_1—C_5 [C_4 lost]	40 14.35	—0.007	+0 56.866	—0.047
" " .	E.	B_1—C_5	42 5.63	—0.005	—0 54.192	—0.042
ϵ Aurigæ	"	B_1—F_5	48 39.08	—0.015	0 0.000	—0.026
11 Orionis . . .	"	B_1—C_5	4 57 16.94	—0.057	0 0.000	—0.057

$T = 4^h$ $\theta = -17^s.8$ $\rho = -0^s.015$ $c = +0^s.050$.

Star.	t	α	Cc	α_0'	$A\alpha$	Δt
α Ceti	2h 55m 38s.45	55m 20s.58	+0s.050	—0s.03	—0s.02	—17s.82
α Persei.	3 15 10.00	14 52.24	+0.076	+0.11	0.00	17.70
γ^2 Ursæ Minoris L. C. .	3 21 9.85	20 52.11	+0.165	—0.11	—0.05	17.86
Groombr. 2320 L. C. .	4 6 11 55	5 53.80	—0.134	—0.08	—0.04	17.84
γ Tauri	12 32.30	12 14.51	—0.052	—0.04	—0.01	17.83
ϵ "	21 9.85	20 52.07	—0.053	—0.03	—0.01	17.82
α "	28 36.13	28 18.35	—0.052	—0.03	—0.01	17.82
α Camelop. U. C. . . .	45 11.28	40 53.39	- - - -	—0.08	+0.01	17.89
ϵ Aurigæ	48 39.05	48 21.13	+0.060	—0.05	—0.01	17.84
11 Orionis	4 57 16.88	56 59.09	+0.052	+0.07	—0.01	—17.71

$$10\,\Delta\theta + 8.063\,a = -0^s.279$$
$$+ 8.063\,\Delta\theta + 15.376\,a = -0.159$$
$$a = -0^s.019,\ \Delta\theta = -0^s.013,\ \Delta t = -17^s\,813.$$

1866, November 16.—M., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
η Aquarii	E.	B_1—F_5	$22^h\,28^m\,49^s.85$	—0ˢ.003	0ᵐ 0ˢ.000	—0ˢ.039
ι Cephei U. C. . . .	"	B_1—F_1	45 31.69	—0.003	—0 14.021	—0.037
α Pegasi	"	B_1—F_5	22 58 27.12	+0.001	0 0.000	—0.009
o Cephei U. C. . . .	"	B_1—F_5	23 12 34.54	+0.008	+0 57.094	—0.010
" "	W.	B_1—F_5	14 28.88	+0.009	—0 57.094	—0.007
θ Piscium	"	B_1—F_5	21 32.18	—0.004	0 0.000	—0.013
λ Draconis L. C. . . .	"	B_1—C_5	24 47.81	—0.020	—0 4.370	—0.043
ι Piscium	"	B_1—F_5	23 23 25.52	—0.050	0 0.000	—0.013

$T = 23^h$ $\theta = -18^s.8$ $\rho = -0^s.013$ $c = 0^s.000$.

Star.	t	a	Cc	ω_0'	Aa	Δt
η Aquarii	$22^h\,28^m\,49^s.81$	$28^m\,31^s.04$	0ˢ.000	+0ˢ.02	—0ˢ.03	—18ˢ.75
ι Cephei U. C. . . .	45 17.63	44 58.85	0.000	+0.01	+0.02	18.77
α Pegasi	22 58 27.11	58 8.35	0.000	+0.04	—0 02	18.78
o Cephei U. C. . . .	23 13 31.70	13 12.94	- - - -	+0.04	+0.03	18.78
θ Piscium	21 32.17	21 13.43	0.000	+0.07	—0.03	18.71
λ Draconis L. C. . . .	23 43.48	23 24.57	0.000	—0.11	—0.10	18.81
ι Piscium	23 33 25.51	33 6.84	0.000	+0.14	—0.03	—18.63

$$7\,\Delta\theta + 4.107\,a = +0^s.215$$
$$+4.107\,\Delta\theta + 9.097\,a = -0.112$$
$$a = -0^s.036,\ \Delta\theta = +0^s.052,\ \Delta t = -18^s.748.$$

V.

OBSERVATIONS AT NEWFOUNDLAND.

Here the Kessels clock was used, and the C. S. transit-instrument No. 6. For determining the intervals of the threads, 100 complete transits of 43 stars are available, which were assorted into seven classes, and the several results of these combined according to weights.

Happily the reticule of this instrument had remained unchanged and unharmed during ten previous longitude-expeditions, for each one of which the equatorial intervals had been carefully determined. The subjoined values are already reduced to the focus used at Heart's Content; those in the first column being derived from eight, and those in the second from two, independent expeditions. In forming the series of adopted values, they received the weights 4 and 1 respectively; the Heart's Content results having the weight 1 also.

EQUATORIAL THREAD-INTERVALS OF TRANSIT No. 6.

	From 8 campaigns before 1859.	Macon and Appalachicola, 1859, 1860.	Heart's Content, 1866.	Adopted.
B_1	+35ˢ.650	+35ˢ.687	+35ˢ.604	+35ˢ.648
B_2	33.120	33.111	33.117	33.118
B_3	30.623	30.614	30.616	30.620
B_4	28.054	28.062	28.052	28.054
B_5	25.437	25.426	25.425	25.433
C_1	20.565	20.552	20.610	20.570
C_2	17.950	17.937	17.954	17.952
C_3	15.407	15.405	15.403	15.406
C_4	12.703	12.695	12.686	12.699
C_5	10.249	10.240	10.228	10 244
D_1	5.100	5.106	5 116	5.104
D_2	2.585	2.590	2.600	2.588
D_3	+0.052	+ 0.068	+ 0.084	+ 0.060
D_4	—2.461	— 2.469	— 2.474	— 2.464
D_5	5.066	5.085	5.078	5.071
E_1	10.112	10.097	10.092	10.106
E_2	12.828	12.837	12.813	12.827
E_3	15.341	15.344	15.343	15.342
E_4	17.969	17.968	17.964	17.968
E_5	20.447	20.460	20.441	20.448
F_1	25.543	25.532	25.535	25.540
F_2	28.120	28.116	28.103	28.116
F_3	30.692	30.691	30.684	30.691
F_4	33.147	33.152	33.134	33.146
F_5	—35.770	—35.746	—35.834	—35.777

The correction for inequality of pivots, resulting from the Newfoundland observations is —0ˢ.019, and since the average value deduced from the last five previous

expeditions was —0ˢ.017, the mean of these, or —0ˢ.018, has been employed, the perforated pivot being the larger.

Although the climate of Newfoundland proved by no means favorable for observation of the heavens, the observers had the satisfaction of obtaining excellent series of transits for time-determinations, both before and after exchanges, upon every date on which longitude-signals were exchanged, whether with Valencia or with Calais. Here, as in the Valencia series, those observations are given, together with their reductions, upon which the longitudes depend; all the transits for Valencia exchanges having been observed by Mr. Dean, and those for the Calais exchanges by Mr. Goodfellow.

1866, October 25.—D., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
$\varkappa$ Cephei U. C. . .	W.	B_1—C_5	20ʰ 11ᵐ 31ˢ.94	—0ˢ.124	+1ᵐ 44ˢ.553	—0ˢ.555
" " .	E.	B_1—C_5	15 0.30	—0.148	—1 44.553	—0.650
α Cygni	"	B_1—F_5	36 49.22	—0.154	0 0.000	—0.236
μ Aquarii	"	C_1—E_5	45 23.66	—0.153	—0 0.028	—0.098
ν Cygni	"	B_1—C_5	20 52 38.64	—0.132	—0 30.200	—0.191
ζ "	W.	B_1—D_5—F_5 [E_5 lost]	21 7 19.11	—0.101	—0 6.990	—0.126
α Cephei	"	B_1—C_5	14 32.13	—0.124	+0 48.960	—0.286
ι Pegasi	"	C_1—F_5	15 59.75	—0.127	—0 8.095	—0.132
24 Urs. Majoris L.C.	"	E_1—F_5	21 23.62	—0.138	+1 8.610	—0.236
" " "	E.	E_1—F_5	23 41.48	—0.138	—1 8.610	+0.236
ξ Aquarii	"	C_1—E_5	30 35.41	—0.142	—0 0.028	+0.094
11 Cephei U. C. . .	"	E_1—F_5	38 45.96	—0.146	+1 9.560	—0.451
" " " . .	W.	E_1—F_5	21 41 5.67	—0.146	—1 9.560	—0.451

$T = 21^h$ $\theta = +4^s.7$. $\rho = 0^s.000$. $c = -0^s.110$.

Star.	t	a	Cc	a_0'	Δa	Δt
$\varkappa$ Cephei U. C. . . .	20ʰ 13ᵐ 15ˢ.52	13ᵐ 20ˢ.51	- - - -	+0ˢ.29	+0ˢ.60	+4ˢ.39
α Cygni	36 48.98	36 53.99	—0ˢ.155	+0.15	—0.02	4.87
μ Aquarii	45 23.54	45 28.08	—0.111	—0.27	—0.23	4.66
ν Cygni	20 52 8.17	52 13.06	—0.145	+0.05	—0.04	4.79
ζ "	21 7 11.99	7 16.62	+0.127	+0.05	—0.10	4.85
α Cephei	15 20.80	15 25.44	+0.234	+0.17	+0.14	4.73
ι Pegasi	15 51.52	15 56.15	+0.116	+0.04	—0.14	4.88
24 Urs. Majoris U. C. .	22 32 79	22 36.65	- - - -	—0.84	—0.71	4.57
ξ Aquarii	30 35 29	30 39.85	—0.111	—0.25	—0.23	4.68
11 Cephei U.C. . . .	21 39 55.36	40 0.40	- - - -	+0.34	+0.32	+4.72

$$10\,\Delta\theta + 1.499\,a = -0^s.260$$
$$+1.499\,\Delta\theta + 15.420\,a = -3.918$$
$$a = -0^s.268,\ \Delta\theta = +0^s.014,\ \Delta t = +4^s.714.$$

1866, October 25.—D., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
γ Cephei U. C. . . .	W.	B_3—C_5	23h 32m 27s.96	—0s.168	+1m 28s.678	—0s.723
" "	E.	B_1—C_5	35 37.32	—0.138	—1 41.250	—0.601
ω Piscium	"	B_1—F_5	23 52 24.79	—0.147	0 0.000	—0.124
α Andromedæ	"	B_1—F_5	0 1 27.22	—0.154	0 0.000	—0.181
γ Pegasi.	"	B_3—F_5	6 19.40	—0.138	0 0.000	—0 133
$\varkappa$ Draconis L. C. . . .	"	B_1—C_5	26 29 25	—0.150	+1 8.916	+0.255
" " . . .	W.	B_1—C_5	29 46.16	—0.152	—1 8.916	+0.255
α Cassiopeæ	"	B_3—E_5	32 42.79	—0.155	+0 13.637	—0.297
β Ceti	"	C_1—E_5	36 50.98	—0.151	+0 0.030	—0.078
32 Camelop. (foll.) L. C.	"	E_1—F_5	44 31.74	—0.133	+2 17.218	+1.007
" " "	E.	D_1—E_3	0 48 37.30	—0.164	—0 46.605	+1.211

$T = 0^h$ $\theta = +4^s.7$ $\rho = 0^s.000$ $c = -0^s.110$.

Star.	t	a	Cc	ω_0'	Aa	Δt
γ Cephei U. C. . . .	23h 33m 56s.34	34m 0s.93	- - - -	+0s.55	+0s.64	+4s.62
ω Piscium	23 52 24 79	52 29.45	—0s.111	—0.03	—0.20	4.87
α Andromedæ	0 1 27.22	1 31 93	—0.125	—0.03	—0.11	4.78
γ Pegasi.	6 19.40	6 24.01	—0.114	—0.07	—0.17	4.80
$\varkappa$ Draconis, L.C.. . .	27 37.70	27 41.86	- - - -	—0.80	—0.78	4.68
α Cassiopeæ	32 56.43	33 0.83	+0.196	+0.20	+0.07	4.82
β Ceti	36 50.96	36 55.11	+0.116	—0.36	—0.29	4.63
32 Camelop. (foll.) L.C. .	0 47 49.83	47 53.52	- - - -	—2.11	—2.16	+4.75

$$8\,\Delta\theta + 10.124\,a = -2^s.651$$
$$+ 10.214\,\Delta\theta + 66.556\,a = -19.293$$
$$a = -0^s.297,\ \Delta\theta = +0^s.045,\ \Delta t = +4^s.745$$

1866, October 28.—D., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
ξ Aquarii . . .	W.	B_1—F_5	21h 30m 34s.77	+0s.011	0m 0s.000	—0s.020
11 Cephei U. C. .	"	B_1—C_5	38 46.28	+0.009	+1 9.493	—0.019
" " " .	E.	B_1—C_5	41 2.59	+0.007	—1 9.493	—0.024
μ Capricorni . .	"	B_1—F_5	45 56.13	+0.005	0 0.000	—0.012
α Aquarii . . .	"	B_1—F_5	21 58 50.94	—0.019	0 0.000	—0.027
π "	"	B_1—F_5	22 18 22.95	—0.019	0 0.000	—0.027
9 Draconis L. C. .	"	$B_{4\,5}$ $C_{4\,5}$ $D_{1\,2\,3}$	22 41.66	—0.009	+0 51.148	+0.081
" " " .	W.	B_1—C_5	25 7.02	—0.005	—1 37.712	+0.071
η Aquarii . . .	"	B_1—F_5	28 26.32	+0.003	0 0.000	—0.012
ζ Pegasi	"	B_1—F_5	34 44.83	+0.004	0 0.000	—0.011
ι Cephei U. C. .	"	B_1—C_5	43 59.64	—0.016	+0 55.394	—0.069
" " . .	E.	B_1—C_5	22 45 48.35	—0.031	—0 55.394	—0.103

$T = 22^h.$ $\quad \theta = +5^s.7.$ $\quad \rho = +0^s.040.$ $\quad c = -0^s.416.$

Star.	t	a	Cc	ω_0'	Aa	Δt
ξ Aquarii . . .	21h 30m 34s.75	30m 39s.80	+0s.421	—0s.21	—0s.04	+5s.54
11 Cephei U. C. .	39 54.41	40 0.20	- - - -	+0.11	+0.06	5.75
μ Capricorni . .	45 56.12	46 2.20	—0.429	—0.04	—0.05	5.71
α Aquarii . . .	21 58 50.91	58 56.96	—0.313	+0.03	—0.04	5.77
π "	22 18 22.92	18 29.02	—0.416	—0.03	—0.04	5.71
9 Draconis L. C. .	23 31.14	23 36.64	- - - -	—0.22	—0.19	5.67
η Aquarii . . .	28 26.31	28 31.31	+0.416	—0.30	—0.04	5.44
ζ Persei	34 44.82	34 49.92	+0.423	—0.19	—0.03	5.54
ι Cephei U. C. .	22 44 53.90	44 59.62	- - - -	—0.02	+0.04	+5.64

$$9\,\Delta\theta + 6.222\,a = +0^s.872.$$
$$+6.222\,\Delta\theta + 17.817\,a = +1.425.$$
$$a = -0^s.053,\ \Delta\theta = -0^s.060,\ \Delta t = +5^s.640.$$

1866, October 28.—D., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
α Cassiopeæ, ...	E.	B_1—F_5	0h 32m 54s.28	—0s.071	0m 0s.000	—0s.149
21 " U.C. . .	"	B_5—C_2	38 6.40	—0.071	—1 13.113	—0.285
ε Piscium	"	B_1—F_5	0 54 57.15	—0.071	0 0.000	—0.068
Polaris U.C. . . .	"	D_3—E_4	1 4 48.10	—0.071	+6 11.875	—2.759
" "	W.	D_5—E_2	17 46.90	—0.004	—6 13.717	—0.687
η Piscium	"	B_1—F_5	24 17.86	—0.032	0 0.000	—0.041
σ "	"	B_1—F_5	38 18.00	—0.076	0 0.000	—0.073
β Arietis.	"	B_1—F_5	47 13.61	—0.095	0 0.000	—0.105
50 Cassiopeæ U.C. . .	"	B_1—C_5	50 55.18	—0.111	+1 13.440	—0.368
" " " . .	E.	B_1—C_5	53 19.08	—0.127	—1 13.440	—0.415
α Arietis	"	B_1—F_5	1 59 35.66	—0.138	0 0.000	—0.151

$T = 1^h$ $\theta = +5^s.8$ $\rho = +0^s.040$ $c = -0^s.416$.

Star.	t	α	Cc	ω_0'	$A\alpha$	Δt
α Cassiopeæ	0h 32m 54s.13	33m 0s.82	—0s.740	+0s.17	+0s.07	+5s.90
21 " U.C. . .	36 53.00	37 0.45	—1.533	+0.13	+0.44	5.49
ε Piscium	0 55 57.08	56 3.10	—0.419	—0.20	—0.18	5.78
Polaris U.C. . . .	1 11 14.87	11 27.75	- - - -	+7.08	+7.13	5.75
η Piscium	24 17.82	24 22.93	+0.430	—0.28	—0.15	5.67
σ "	38 17.93	38 23.16	+0.421	—0.18	—0.17	5.79
β Arietis	47 13.51	47 18.73	+0.442	—0.17	—0.13	5.77
50 Cassiopeæ U.C. . .	52 7.06	52 12.79	- - - -	—0.11	+0.35	5.34
α Arietis.	1 59 35.51	59 41.80	+0.451	0.00	—0.12	+5.92

$$9\,\Delta\theta - 26.964\,a = +6^s.439$$
$$-26.964\,\Delta\theta + 713.993\,a = -188.899$$
$$a = -0^s.268,\ \Delta\theta = -0^s.087,\ \Delta t = +5^s.713.$$

1866, November 5.—D., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
9 Draconis L.C.	W.	E_1—F_5	22h 22m 51s.74	—0s.038	+1m 37s.810	+0s.150
" " "	E.	E_1—F_5 [F_2 lost]	24 49.73	—0.044	—1 20.762	+0.164
ζ Pegasi	"	B_1—F_5	34 41.58	—0.061	0 0.000	—0.063
ι Cephei U.C.	"	E_1—F_5	43 55.93	—0.032	+0 55.442	—0.106
" " "	W.	E_1—F_5	45 46.44	—0.016	—0 55.442	—0.069
α Pegasi	"	D_2—F_5 [D_2 lost]	22 58 18.68	—0.036	—0 18.462	—0.046
ι Piscium	"	C_2—F_5	23 33 7.81	—0.063	—0 9.162	—0.060
γ Cephei U.C.	"	E_2—F_5	35 46.01	—0.063	—1 54.045	—0.304
ω Piscium	E.	B_1—F_5	23 52 21.10	—0.136	0 0.000	—0.116
α Andromedæ	"	B_1—F_5	0 1 23.60	—0.018	0 0.000	—0.035
γ Pegasi	W.	B_1—F_5	0 6 15.69	0.000	0 0.000	—0.014

$T = 23^h$ $\theta = +8^s.3$ $\rho = 0^s.000$ $c = +0^s.033$.

Star.	t	α	Cc	ω_0'	$A\alpha$	Δt
9 Draconis L.C.	22h 23m 42s.42	23m 37s 34	- - - -	—0s.43	—0s.34	+8s.21
ζ Pegasi	34 41.52	34 49.79	+0s.034	+0 01	—0.06	8.37
ι Cephei U.C.	44 51.10	44 59.32	- - - -	—0.08	+0.07	8.15
α Pegasi	22 58 0.17	58 8.49	—0.034	—0.02	—0.06	8.34
ι Piscium	23 32 58.59	33 6.95	—0.033	+0 03	—0.07	8.40
γ Cephei U.C.	33 51.66	34 0.30	—0.147	+0.19	+0.21	8.28
ω Piscium	23 52 20.95	52 29.38	+0.033	+0.16	—0.07	8.53
α Andromedæ	0 1 23.57	1 31.86	+0.038	+0.03	—0.04	8.37
γ Pegasi	0 6 15.68	6 23.94	—0.034	—0.07	—0.06	+8.29

$$9\,\Delta\theta + 4.140\,a = -0^s.170.$$
$$+4.140\,\Delta\theta + 19.703\,a = -1.755.$$
$$a = -0^s.171,\ \Delta\theta + 0^s.026,\ \Delta t = +8^s.326.$$

1866, November 5.—D., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
θ' Ceti	W.	B_1—F_5	1h 17m 14s.92	—0s.101	0m 0s.000	—0s.070
A Cassiopeæ U.C. . .	"	B_1—C_5	20 12.71	—0.098	+1 5.828	—0.301
" " " . .	E.	B_1—C_5	22 24.44	—0.098	—1 5.828	—0.301
η Piscium	"	B_1—E_5	24 22.66	—0.095	—0 7.922	—0.096
o "	"	B_1—F_5	38 14.92	—0.104	0 0.000	—0.095
β Arietis	"	B_1—F_5	47 10.56	—0.107	0 0.000	—0.116
50 Cassiop. U.C. . . .	"	E_1—F_2	50 51.22	—0.106	+1 13.510	—0.354
" " " . .	W.	E_1—F_5	53 18.13	—0.103	—1 13.510	—0.345
α Arietis	"	B_1—F_5	1 59 33.58	—0.099	0 0.000	—0.112
65 Ceti	"	B_1—F_5	2 5 49.83	—0.087	0 0.000	—0.082
ϵ Cassiopeæ U.C. . .	"	B_1—C_5	17 5.84	—0.103	+0 58.307	—0.282
" " " . . .	E.	B_1—C_5	19 2.63	—0.123	—0 58.307	—0.331
5 Urs. Minoris L. C. .	"	B_1—C_5	25 58.14	—0.126	+1 36.946	+0.357
" " " .	W.	B_1—D_3	2 28 52.39	—0.114	—1 17.092	+0.327

$T = 2^h$ $\theta = +8^s.3$ $\rho = 0^s.000$ $c = +0^s.033$.

Star.	t	α	Cc	α_0'	Aa	Δt
θ' Ceti	1h 17m 14s.85	17m 23s.11	—0s.034	—0s.07	—0s.16	+8s.38
A Cassiopeæ U.C. . .	21 18.27	21 26.77	- - - -	+0.20	+0.20	8.30
η Piscium	24 14.64	24 22.93	+0.034	+0.02	—0.11	8.43
o "	38 14.82	38 23.18	+0.034	+0.09	—0.12	8.51
β Arietis	47 10.44	47 18.76	+0.036	+0.05	—0.09	8.44
50 Cassiopeæ U. C. . .	52 4.33	52 12.81	- - - -	+0.18	+0.24	8.24
α Arietis	1 59 33.47	59 41.84	—0.036	+0.04	—0.09	8.42
65 Ceti	2 5 49.75	5 57.95	—0.034	—0 13	—0.12	8.29
ϵ Cassiopeæ U.C. . .	18 3.93	18 12.24	- - - -	+0.01	+0.15	8.16
5 Urs. Minoris L.C. . .	2 27 35.53	27 43.04	- - - -	—0.79	—0.66	+8.16

$$10\,\Delta\theta + 3.969\,a = -0^s.427$$
$$+3.969\,\Delta\theta + 17.993\,a = -3.258$$
$$a = -0^s.188,\ \Delta\theta = +0^s.032,\ \Delta t = +8^s.332.$$

1866, November 6.—D., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
α^2 Capricorni	W.	B_1—C_5	20h 10m 7s.83	+0s.087	+0m 23s.574	+0s.030
$\varkappa$ Cephei U.C.	"	B_4—D_8	12 8.24	+0.061	+1 2.852	+0.179
" " "	E.	B_1—C_5	14 55.99	+0.037	—1 44.555	+0.084
π Capricorni	"	$B_{1\,2\,3}$ E_1—$_5$	19 36.36	+0.006	—0 2.994	—0.012
ε Delphini	"	B_1—F_5	26 42.81	—0.032	0 0.000	—0.040
Groombr. 3241 U.C. .	"	C_5—F_5	29 41.77	—0 038	+0 44.580	—0.174
α Cygni.	"	B_1—F_5	36 45.74	—0.059	0 0.000	—0.102
μ Aquarii	"	B_1—F_5	45 20.03	—0.079	0 0.000	—0.057
12–Y. Cat. 1879 U. C. .	"	D_3—F_8	52 6.36	—0.059	+1 19.774	—0.369
" " "	W.	E_1—F_5	20 55 39 03	—0.040	—2 13.109	—0.376
ζ Cygni	"	B_1—F_5	21 7 8.38	—0.002	0 0.000	—0.018
$\dot{\alpha}$ Cephei	"	B_1—C_5	14 28.01	—0.026	+0 48.960	—0.082
1 Pegasi	"	C_3—E_5	15 50.96	—0.038	—0 3.106	—0.049
24 Ursæ Majoris L. C. .	"	E_1—F_5	21 21.51	—0.074	+1 8.610	+0.146
" " " .	E.	E_1—F_5	21 23 38.33	—0.088	—1 8.610	+0.165

$T = 21^h$ $\theta = +7^s.9$ $\rho = +0^s.030$ $c = +0^s.033$.

Star.	t	a	Cc	a_0'	Aa	Δt
α^2 Capricorni	20h 10m 31s.43	10m 39s.38	—0s.034	+0s.04	—0s.06	+8s.00
$\varkappa$ Cephei U. C. . . .	13 11.39	13 19.24	- - - -	—0.08	+0.16	7.71
π Capricorni	19 33.35	19 41.32	+0.035	+0.12	—0.07	8.09
ε Delphini	26 42.77	36 50.83	+0.034	+0.21	—0.04	8.15
Groombr. 3241 U. C. .	30 26.18	30 34.07	+0.108	+0.12	+0.09	7.92
α Cygni	36 45.64	36 53.67	+0.037	+0.18	—0 01	8.09
μ Aquarii	45 19.97	45 27.89	+0.034	+0.06	—0.06	8 02
12–Y. Cat. 1879 U. C. .	20 53 25.70	53 33.91	- - - -	+0.31	+0.22	7.99
ζ Cygni	21 7 8 36	7 16.39	—0.038	+0.09	—0.03	8.01
α Cephei U. C. . . .	15 16.89	15 24.93	—0.071	+0.06	+0.04	7.93
1 Pegasi	15 47.80	15 55.95	—0.035	+0.20	—0.04	8.14
24 Ursæ Majoris L. C. .	21 22 30.07	22 37.52	- - - -	—0.47	—0.18	+7.62

$$12\,\Delta\theta - 0.272\,a = +0^s.889$$
$$+0.272\,\Delta\theta + 26.635\,a = -1.844$$
$$a = -0^s.070,\ \Delta\theta = +0^s.073,\ \Delta t = +7^s.973.$$

1866, November 6.—D., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
ϵ Cephei U. C. . . .	E.	E_1—F_5	$22^h\,43^m\,56^s.08$	$-0^s.003$	$+0^m\,55^s.448$	$-0^s.039$
" "	W.	E_1—F_5	45 46.76	0.000	−0 55.448	−0.032
α Pegasi.	"	B_1—F_5	22 58 0.52	−0.056	0 0.000	−0.062
o Cephei U. C. . . .	"	B_1—C_5	23 12 5.69	−0.069	+0 59.729	−0.205
" "	E.	B_1—C_8	14 12.84	−0.057	−1 7.205	−0.176
θ Piscium	"	C_1—F_5	20 58.08	−0.072	+0 7.681	−0.068
λ Draconis L. C. . . .	"	C_1—F_5	23 38.76	−0.092	−0 22.424	+0.168
ϵ Piscium	"	B_1—F_5	32 59.08	−0.126	0 0.000	−0.107
ω "	"	B_1—F_5	23 52 21.48	−0.162	0 0.000	−0.026
α Andromedæ	W.	B_1—F_5	1 23.93	−0.128	0 0.000	−0.153
γ Pegasi	"	B_1—F_5	6 16.04	−0.097	0 0.000	−0.098

$T = 23^h$ $\quad \theta = +8^s.0$ $\quad \rho = +0^s.030$ $\quad c = +0^s.033.$

Star.	t	α	Cc	α_0'	$A\alpha$	Δt
ϵ Cephei U.C. . . .	$22^h\,44^m\,51^s.38$	$44^m\,59^s.28$	- - - -	$-0^s.10$	$+0^s.12$	$+7^s.79$
α Pegasi	22 58 0.46	58 8.48	$-0^s.034$	−0.01	−0.09	8.08
o Cephei U.C. . . .	23 13 5.34	13 13.36	- - - -	+0.02	+0.14	7.88
θ Piscium	21 5.69	21 13 55	+0.033	−0.12	−0.11	7.99
λ Draconis L.C. . . .	23 16.50	23 23.96	−0.098	−0.66	−0.42	7.76
ϵ Piscium	32 58.97	33 6.94	+0 033	−0.02	−0.11	8.10
ω "	23 52 21.45	52 29.37	+0 033	−0.08	−0.11	8.03
α Andromedæ	1 23.78	1 31.85	−0.038	0.00	−0.06	8.07
γ Pegasi	6 15.94	6 23.93	−0.035	−0.08	−0.08	+8.00

$$9\,\Delta\theta + 4.540\,a = -1^s.034.$$
$$+4.540\,\Delta\theta + 10.441\,a = -1.845.$$
$$a = -0^s.162,\ \Delta\theta = -0^s.033,\ \Delta t = +7^s.967.$$

1866, November 9.—D., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
Groombr. 3241 U.C.	W.	B_1—E_4 [$D_{4,5}$ lost]	20h 30m 18s.36	—0s.055	+0m 7s.685	—0s.208
α Cygni	"	B_4—D_4 [D_4 lost]	36 27.42	—0.055	+0 18.397	—0.096
μ Aquarii . . .	"	B_2—F_5 [$D_{1,2}$ lost]	45 20.75	—0.055	—0 0.339	—0.044
ν Cygni	"	B_1—F_5	52 4.93	—0.071	0 0.000	—0.111
σ^2 Urs. Majoris U.C.	"	D_3—E_5	20 58 1.96	—0.095	+0 27.697	+0.144
ζ Cygni	E.	B_1—F_5	21 5 8.63	—0.121	0 0.000	—0.148
α Cephei	"	D_1—F_5	14 44.56	—0.175	+0 32.640	—0.392
24 Ursæ Majoris U.C.	"	B_1—C_5	21 21.51	—0.143	+1 8.544	+0.244
" " "	W.	B_1—D_3	23 29.05	—0.107	—0 59.024	+0.193
β Cephei U.C. . .	"	E_1—F_5	27 56.94	—0.105	—1 7.150	—0.324
ξ Aquarii . . .	"	B_1—F_5	30 32.07	—0.113	0 0.000	—0.077
11 Cephei U.C. . .	"	B_1—C_5	38 42.40	—0.141	+1 9.493	—0.437
" " . .	E.	B_2—C_5	41 1.54	—0.153	—1 9.493	—0.471
μ Capricorni . .	"	B_1—F_5	45 54.36	—0.163	0 0.000	—0.093
79 Draconis . .	"	E_1—F_5	49 48.56	—0.170	+1 8.962	—0.575
" " ". .	W.	D_3—F_5	21 52 10.23	—0.170	—1 2.713	—0.575

$T = 2^h$ $\theta = +7^s.8$ $\rho = 0^s.000$ $c = +0^s.033$.

Star.	t	α	Cc	α_0'	$A\alpha$	Δt
Groombr. 3241 U.C. .	20h 30m 25s.84	30m 33s.85	—0s.108	+0s.11	+0s.12	+7s.78
α Cygni	36 45.72	36 53.58	—0.047	+0.01	—0.01	7.82
μ Aquarii	45 20.37	45 27.94	—0.034	—0.26	—0.08	7.62
ν Cygni	52 4.82	52 12.70	—0.044	+0.04	—0.01	7.85
σ^2 Ursæ Majoris L.C. .	20 58 29.80	58 37.35	+0.088	—0.16	—0.22	7.86
ζ Cygni	21 5 8.48	5 16.33	+0.038	+0.09	—0.03	7.92
α Cephei	15 16.81	15 24.80	+0.071	+0.26	+0.05	8.02
24 Ursæ Majoris L.C. .	22 30.26	22 37.75	- - - -	—0.31	—0.24	7.73
β Cephei U.C. . . .	26 49.47	26 57.28	—0.097	—0.09	+0.10	7.61
ξ Aquarii	30 31.99	30 39.63	—0.034	—0.20	—0.08	7.68
11 Cephei U.C. . . .	39 51.52	39 59.49	- - - -	+0.17	+0.11	7.87
μ Capricorni	45 54.27	46 2.02	+0.035	—0.01	—0.08	7.87
79 Draconis	21 51 6.94	51 14.98	- - - -	+0.23	+0.13	+7.90

$$13\,\Delta\theta + 2.620\,a = -0^s.125.$$
$$+2.620\,\Delta\theta + 21.728\,a = -2.008.$$
$$a = -0^s.093,\ \Delta\theta + 0^s.010,\ \Delta t = +7^s.810.$$

1866, November 9.—D., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
β Ceti	W.	B_1—F_5	0h 36m 47s.47	—0s.138	0m 0s.000	—0s.073
32 Camelop. (foll.) L.C.	"	D_2—F_2	45 33.60	—0.156	+2 15.030	+1.158
" " "	E.	E_1—F_5	51 32 00	—0.173	—3 45.318	+1.270
ε Piscium	"	B_1—F_5	0 55 55.58	—0.185	0 0.000	—0.155
ο "	"	B_1—F_5	1 38 15.55	—0.233	0 0.000	—0.196
β Arietis	"	B_1—F_5	47 11.24	—0.206	0 0.000	—0.209
50 Cassiopeæ U. C.	"	E_1—F_5	50 52.01	—0.188	+1 13.510	—0.594
" " "	W.	E_1—F_5	53 18.82	—0.180	—1 13.510	—0.570
α Arietis	"	B_1—F_5	1 59 34.22	—0.186	0 0.000	—0.198
65 Ceti	"	B_1—F_5 [B_4 C_5 F_4 lost]	2 5 52.21	—0.221	—0 1.838	—0.186
ι Cassiopeæ U.C.	"	B_1—C_5 [B_3 lost]	17 8.71	—0.203	—0 0.203	—0.522
" " "	E.	B_1—C_5 [C_1 lost]	2 18 4.13	—0.195	—0 0.195	—0.503

$T = 1^h$ $\theta = +7^s.8$ $\rho = 0^s.000$ $c = +6^s.033$.

Star.	t	a	Cc	a_0'	Aa	Δt
β Ceti	0h 36m 47s.40	36m 55s.04	—0s.035	—0s.19	—0s.23	+7s.84
32 Camelop. (foll.) L.C.	47 48.87	47 54.84	- - - -	—1.83	—1.75	7.72
ε Piscium	55 55.42	56 3.07	+0.033	—0.12	—0.16	7.84
ο "	1 38 15.35	38 23.18	+0.034	+0.06	—0.15	8.01
β Arietis	47 11.03	47 18.76	+0.035	—0.04	—0.12	7.88
50 Cassiopeæ U.C.	52 4.83	52 12.80	- - - -	+0.17	+0.31	7.66
α Arietis	1 59 34.02	59 41 85	—0.036	—0.01	—0.07	7.86
65 Ceti	2 5 50.19	5 57.96	—0.034	—0.06	—0.16	7.90
ι Cassiopeæ U.C.	2 18 4.49	18 12.26	- - - -	—0.03	—0.20	+7.58

$$9\,\Delta\phi + 9.039\,a = -2^s.050.$$
$$+9.039\,\Delta\theta + 58.021\,a = -13.809.$$
$$a = -0^s.240,\ \Delta\theta = +0^s.013,\ \Delta t = +7^s.813.$$

1866, December 11.—Gr., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
9 Draconis L.C. .	E.	B_1—C_5	22h 22m 1s.36	—0s.099	+1m 37s.712	+0s.296
" " . .	W.	B_1—D_5 [D_4 lost]	24 49.70	—0.089	—1 10.610	+0.272
ζ Pegasi	"	E_8—$_5$	35 4.27	—0.054	—0 16.909	—0.057
ι Cephei U.C. . .	"	B_1—F_5	44 55.55	—0.068	0 0.000	—0.188
α Pegasi	"	C_1—C_4	22 57 48.82	—0.084	+0 17.202	—0.094
θ Piscium . . .	"	B_1—F_5	23 21 11.16	—0.108	0 0.000	—0.094
γ Cephei U.C. . .	"	B_1—C_5	32 14.29	—0.121	+1 41.250	—0.528
" " . . .	E.	B_1—C_5	35 37.21	—0.127	—1 41.250	—0.550
Groombr. 4163 U.C.	"	B_4—F_5	48 24.20	—0.144	0 0.000	—0.511
ω Piscium . . .	"	B_1—E_5	23 52 34.61	—0.148	—0 7.708	—0.125
α Andromedæ . .	"	C_1—E_5	1 29.34	—0.147	—0 0.032	—0.173
4 Draconis L.C. .	"	B_1—C_5	3 55.23	—0.145	+1 53.865	+0.493
" " . .	W.	B_1—D_5	7 5.11	—0.141	—1 15.985	+0.481

$T = 23^h$ $\theta = +2^s.1$ $\rho = -0^s.02$ $c = +0^s.033$.

Star.	t	α	Cc	ω_0'	$A\alpha$	Δt
9 Draconis L. C. . . .	22h 23m 39s.37	23m 41s.18	- - - -	—0s.30	—0s.36	+2s.17
ζ Pegasi.	34 47.30	34 49.32	—0s.034	—0.13	—0.06	2 04
ι Cephei U.C.	44 55.36	44 57.72	—0.080	+0.17	+0.08	2.20
α Pegasi	22 58 5.93	58 8.03	—0.034	—0.03	—0.06	2.13
θ Piscium	23 21 11.07	21 13.13	—0.033	—0.06	—0.07	2.11
γ Cephei U.C.	33 55.21	33 57.53	- - - -	+0.22	+0.22	2.10
Groombr. 4163 U.C. . .	48 23.69	48 26.12	+0.119	+0.47	+0.16	2.40
ω Piscium	23 52 27.03	52 29.00	+0.033	—0.08	—0.07	2.09
α Andromedæ	1 29.14	1 31.43	+0.038	+0.24	—0.04	2.38
4 Draconis L. C. . . .	5 49.60	5 51.42	- - - -	—0.25	—0.42	+2.26

$$10\,\Delta\theta + 6.015\,a = +0^s.257.$$
$$+6.015\,\Delta\theta + 37.765\,a = -3.483.$$
$$a = -0^s.104,\ \Delta\theta = +0^s.088,\ \Delta t = +2^s.188.$$

1866, December 11.—Gf., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
δ Draconis L.C. . . .	W.	E_1—F_5	7h 11m 26s.58	—0s.137	+0m 59s.903	+0s.188
" "	E.	D_2—F_5	13 13.84	—0.151	—0 47.577	+0.204
β Geminorum	"	B_1—F_5	37 9.45	—0.223	0 0.000	—0.255
φ "	"	D_1—F_5	45 3.06	—0.208	+0 17.205	—0.235
ε Draconis L.C. . . .	"	B_1—C_5	47 25.07	—0.197	+1 6.936	+0.308
" "	W.	B_1—D_2	49 25.45	—0.187	—0 53.228	+0.294
3 Ursæ Majoris U.C. .	"	B_1—D_5	7 58 49.83	—0.154	+0 42.513	—0.437
Groombr. 3241 L.C. . .	"	C_1—E_5	8 30 30.04	—0.107	—0 0.091	+0.219
ε Hydræ	"	B_1—F_5	8 39 42.64	—0.103	0 0.000	—0.092
ϰ Cancri	"	B_1—F_2	9 0 31.11	—0.129	0 0.000	—0.120

$T = 8^h$ $\theta = +1^s.9$ $\rho = -0^s.020$ $c = +0^s.033$.

Star.	t	α	Cc	α_0'	Aa	Δt
δ Draconis L.C. . . .	7h 12m 26s.54	12m 28s.33	- - - -	—0s.12	—0s.28	+2s.06
β Geminorum	37 9.20	37 11.34	+0s.038	+0.27	—0.05	2.22
φ "	45 20.03	45 22.13	+0.038	+0.23	—0.05	2.18
ε Draconis L.C. . . .	48 32.41	48 34.09	- - - -	—0.23	—0.31	1.98
3 Ursæ Majoris U.C. .	7 59 31.94	59 34.07	—0.092	+0.13	+0.12	1.91
Groombr. 3241 L.C. . .	8 30 30.17	30 31.77	+0.108	—0.18	—0.34	2.06
ε Hydræ	8 39 42.55	39 44.53	—0.033	+0.06	—0.08	2.04
ϰ Cancri	9 0 30.99	0 32.95	—0.034	+0.06	—0.07	+2.03

$$8\,\Delta\theta + 8.801\,a = +0^s.227.$$
$$+8\,801\,\Delta\theta + 22.225\,a = -1.252.$$
$$a = -0^s.120,\ \Delta\theta = +0^s.160,\ \Delta t = +2^s.060.$$

1866, December 12.—Gr., Obs.

Star.	Lamp.	Threads.	*M*	b_0	*R*	Bb_0+k
θ' Ceti	W.	B_1—F_5	1h 17m 21s.45	+0s.077	0m 0s.000	+0s.028
A Cassiopeæ U. C.	"	B_1—C_5	20 17.80	+0.074	+1 5.828	+0.157
η Piscium . . .	"	C_1—F_5	24 29.23	+0.068	—0 7.901	+0.045
β Arietis	"	B_1—C_3 E_3—F_4	1 47 15.58	+0.061	+0 1.533	+0.043
ζ Urs. Minoris L.C.	"	E_1—F_3	3 47 5.96	+0.009	+1 38.478	+0 042
" " "	E.	D_1—F_5 [F_4 lost]	3 49 55.34	—0.001	—1 11.358	+0.071
γ Tauri	"	B_1—F_5	4 12 13.38	—0.039	0 0.000	—0.048
ϵ "	"	B_1—F_5	20 50.98	—0 037	0 0.000	—0.049
α "	"	B_1—F_5	28 17.19	—0.030	0 0.000	—0.041
α Camelop. U. C. .	"	E_1—F_5	39 55.70	+0.002	+0 56.784	—0.029
" " .	W.	D_3—F_5	4 41 37.50	+0.008	—0 45.099	—0.016

$T = 3^h$ $\theta = +1^s.6$ $\rho = 0^s.000$ $c = +0^s.033$.

Star.	*t*	α	*Cc*	ω_0'	*Aa*	Δt
θ' Ceti.	1h 17m 21s.48	17m 22s.90	—0s.034	—0s.21	—0s.22	+1s.61
A Cassiopeæ U.C.	21 23.78	21 25.82	—0.096	+0.34	+0.28	1.66
η Piscium . . .	24 21.37	24 22.75	—0.035	—0.26	—0.15	1.49
β Arietis. . . .	1 47 17.16	47 18.66	—0.036	—0.13	—0 13	1.60
ζ Urs. Minoris L.C.	3 48 44.07	48 44.68	- - - -	—0.99	—1.03	1.65
γ Tauri	4 12 13.33	12 14.80	+0.035	—0.10	—0.15	1.65
ϵ "	20 50.93	20 52.38	+0.035	—0.12	—0.13	1.62
α "	28 17.15	28 18.68	+0.035	—0.03	—0.14	1.71
α Camelop. U.C. .	4 40 52.42	40 54.11	- - - -	+0.09	+0.20	+1.49

$$9\,\Delta\theta + 5.646\,a = -1^s.407$$
$$+5.646\,\Delta\theta + 19.478\,a = -4.845$$
$$a = -0^s.261,\ \Delta\theta = +0^s.007,\ \Delta t = +1^s.607.$$

1866, December 12.—Gr., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
ε Hydræ	W.	B_1—F_5	8h 39m 43s.37	—0s.079	0m 0s.000	—0s.074
σ^3 Urs. Majoris U. C. .	"	B_1—C_5	57 37.69	—0.113	+1 0.481	—0.316
" " . .	E.	B_1—D_3	8 59 26.54	—0.116	—0 48.095	—0.323
1 Draconis U. C. . . .	"	D_1—F_5	9 16 3.71	—0.117	+1 48.775	—0.787
α Hydræ	"	B_1—F_5	21 2.38	—0.107	0 0.000	—0.075
θ Ursæ Majoris . . .	"	E_1—F_5	23 17.91	—0.104	+0 37.589	—0.192
β Cephei L. C.	"	B_3—C_5	25 55 51	—0.100	+0 58.759	+0.176
" "	W.	B_1—C_5	28 1.60	—0.094	—1 7.086	+0.168
ε Leonis	"	B_1—F_5	38 16.67	—0.083	0. 0.000	—0.099
μ "	"	B_1—E_5	9 45 0.38	—0.083	+0 10.228	—0.103

$T = 9^h$ $\theta = +1^s.6$ $\rho = 0^s.000$ $c = +0^s.033$.

Star.	t	a	Cc	ω_0'	Aa	Δt
ε Hydræ	8h 39m 43s.30	39m 44s.48	—0s.033	—0s.45	—0s.18	+1s.33
σ^3 Urs. Majoris U. C. .	8 58 37.99	58 39.72	- - - - -	+0.13	+0.24	1.49
1 Draconis U. C. . . .	9 17 51.70	17 53.88	+0.237	+0 82	+0.07	1.34
α Hydræ	21 2.30	21 3.70	+0.034	—0.17	—0.23	1.65
θ Ursæ Majoris . . .	23 55.31	23 56.88	+0.054	+0.03	+0.03	1.59
β Cephei L C.	26 54.56	26 55.31	- - - - -	—0.75	—0.70	1.54
ε Leonis	38 16.57	38 18.08	—0.036	—0.13	—0.12	1.59
μ "	9 45 10.52	45 11.77	—0.037	—0.39	—0.11	+1.32

$$8\,\Delta\theta - 0.076\,a = -0^s.916$$
$$+0.076\,\Delta\theta + 24.801\,a = -5.712$$
$$a = -0^s.270,\ \Delta\theta = -0^s.117,\ \Delta t = +1^s.483.$$

1866, December 14.—Gr., Obs.

Star.	Lamp.	Threads.	*M*	b_0	*R*	Bb_0+k
9 Draconis L. C. .	W.	E_1—F_5	22h 22m 4s.27	—0s.029	+1m 37s.808	+0s.128
" " . .	E.	D_2—F_5 [F_1 lost]	24 56.68	—0.035	—1 34.548	+0.143
ε Cephei U. C. . .	"	E_1—F_5 [E_2 lost]	22 43 59.64	—0.059	+0 58.181	—0.168
ο Piscium . . .	"	B_1—F_5	1 38 23.14	—0.024	0 0.000	—0.033
β Arietis	W.	B_1—F_5	47 18.78	—0.022	0 0.000	—0.035
α "	"	B_1—F_5	1 59 41.81	—0.010	0 0.000	—0.025
α Persei	"	C_1—F_5	3 15 4.22	—0.074	—0 11.741	—0.135
γ^2 Urs. Minoris L C.	"	E_1—F_5	19 37.46	—0.104	+1 15.680	+0.217
" " " .	E.	D_2—F_5	21 48 14	—0.124	—0 55.205	+0.250
η Tauri	"	C_1—E_5	39 36.36	—0.156	—0 0.031	—0 170
ζ Persei	"	C_1—E_5	45 48.38	—0.134	—0 0 033	—0.167
ζ Urs. Minoris L.C.	"	C_1—C_5	47 29 83	—0.130	+1 15.212	+0.442
" " "	W.	B_1—D_4	3 50 7.21	—0.120	—1 22.130	+0.414
Groombr. 2320 L.C.	"	B_1—F_5 [D_3 lost]	4 5 55.28	—0.127	—0 0 567	+0.187
γ Tauri	"	B_1—F_5	12 15.11	—0.145	0 0.000	—0.141
ε "	"	B_1—F_5	20 52.79	—0.167	0 0.000	—0.169
α "	E.	B_1—F_5	4 28 18.90	—0.183	0 0.000	—0.176

$T = 2^h$ $\theta = -0^s.05$ $\rho = -0^s.030$ $c = +0^s.033$.

Star.	*t*	*a*	*Cc*	ω_0'	*Aa*	Δt
9 Draconis L. C. .	22h 23m 42s.09	23m 41s.43	- - - -	—0.s72	—0s.81	+0s.04
ε Cephei U. C. . .	22 44 57.66	44 57.59	+0s.080	—0.04	+0.17	—0.25
ο Piscium . . .	1 38 23.11	38 23.02	+0.034	—0.02	—0.15	+0.08
β Arietis	47 18.74	47 18.64	—0.036	—0.10	—0.12	—0.03
α "	1 59 41.79	59 41.75	—0.036	—0.02	—0.11	+0.04
α Persei	3 14 52.34	14 52.34	—0.051	+0.03	+0.01	—0.03
γ^2 Urs. Minoris L.C.	20 53.27	20 52.63	- - - - - -	—0.55	—0.66	+0.06
η Tauri	39 36.16	39 36.18	+0.036	+0.16	—0.10	+0.21
ζ Persei	45 48.18	45 48.02	+0.039	—0.02	—0.08	+0.01
ζ Urs. Minoris L.C.	3 48 45.49	48 44.72	- - - - -	—0 67	—0.91	+0.20
Groombr. 2320 L.C.	4 5 54.90	5 53.89	+0.089	—0.81	—0.56	—0.30
γ Tauri	12 14.97	12 14.80	—0.035	—0.09	—0.13	—0.01
ε "	20 52.62	20 52.40	—0.035	—0.14	—0.12	—0.07
α "	4 28 18.72	28 18.70	+0.038	+0.14	—0.12	+0.21

$$14\,\Delta\theta + 15.950\,a = -2^s.826$$
$$+15.950\,\Delta\theta + 44.430\,a = -8.695$$
$$a = -0^s.231,\ \Delta\theta = +0^s.061,\ \Delta t = +0^s.011.$$

1866, December 14.—Gr., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
γ Geminorum . . .	E.	B_1—F_5	$6^h\,30^m\,3^s.18$	$-0^s.207$	$0^m\,0^s.000$	$-0^s.158$
51 Cephei U C. . . .	"	D_1—E_5	36 30.90	−0.222	+1 7.471	−3.856
" " " . . .	W.	E_1—F_5	45 31.60	−0.244	−7 58.244	−4.209
ϵ Canis Majoris . . .	"	C_1—E_5	6 53 25.97	−0.273	+0 0.032	−0.088
δ " " . . .	"	B_1—F_5	7 3 0.98	−0.307	0 0.000	−0.109
δ Geminorum . . .	"	B_1—F_5	12 12.25	−0.318	0 0.000	−0.325
τ Draconis L. C. . . .	"	D_1—E_5	17 36.76	−0.318	+0 26.313	+0.611
" " " . . .	E.	E_4—F_5	19 36.74	−0 319	−1 34.226	+0.613
β Geminorum . . .	"	B_1—F_5	37 11.91	−0.427	0 0.000	−0.473
ϕ "	"	C_1—E_5	45 22.93	−0.399	−0 0.031	−0.435
ϵ Draconis L.C. . . .	"	B_1—C_5	47 27.73	−0.395	+1 6.936	+0.577
" " " . . .	W.	B_1—D_5	7 49 27.95	−0.389	−0 53.228	+0.568

$T = 7^h$ $\theta = -0^s.20$ $\rho = -0^s.03$ $c = +0^s.033$.

Star.	t	a	Cc	a_0'	Aa	Δt
γ Geminorum . . .	$6^h\,30^m\,30^s.22$	$30^m\,2^s.84$	$+0^s.035$	$+0^s.04$	$-0^s.14$	$-0^s.02$
51 Cephei U. C. . . .	37 31.83	37 35.04	- - - -	+3.39	+3.51	−0.32
ϵ Canis Majoris . . .	6 53 25.91	53 25.41	−0.038	−0.34	−0.30	−0.25
δ " " . . .	7 3 0.87	3 0.42	−0.037	−0.29	−0.29	−0.20
δ Geminorum . . .	12 11.92	12 11.75	−0.036	+0.01	−0.12	−0.07
τ Draconis L.C. . . .	18 3.41	18 2.16	- - - -	−1.04	−0.79	−0.45
β Geminorum . . .	37 11.44	37 11.41	+0.038	+0.23	−0.10	+0.13
ϕ "	45 22.49	45 22 21	−0.037	−0.02	−0.11	−0.12
ϵ Draconis L. C. . . .	7 48 35.26	48 34.00	- - - -	−1.04	−0.69	−0.55

$$9\,\Delta\theta - 3.692\,a = +0^s.932.$$
$$-3.692\,\Delta\theta + 192.501\,a = -51.059.$$
$$a = -0^s.266,\ \Delta\theta = -0^s.006,\ \Delta t = -0^s.206.$$

1866, December 16.—Gr., Obs.

Star.	Lamp.	Threads.	*M*	b_0	*R*	Bb_0+k
o Piscium	W.	B_1—F_5	1h 38m 24s 26	+0s.144	0m 0s.000	+0s.098
β Arietis	"	B_1—F_5	47 19.80	+0.118	0 0.000	+0.096
50 Cassiopeæ U.C. . .	"	B_1—C_5	50 58.54	+0.097	+0 13.440	+0.240
" " " . . .	E.	B_1—D_4	53 6.10	+0 087	—0 53.665	+0.210
α Arietis	"	B_1—F_5	1 59 42.86	+0.073	0 0 000	+0.057
65 Ceti	"	B_1—F_5	2 5 59.16	+0.069	0 0 000	+0.040
ι Cassiopeæ U.C. . . .	"	E_1—F_5	17 14.38	+0.093	+0 58 363	+0.188
" " . . .	W.	D_3—F_5	2 18 58.85	+0.114	—0 46.353	+0.239

$T = 2^h$ $\theta = -1^s.10$ $\rho = 0^s.000$ $c = +0^s.033$.

Star.	*t*	α	*Cc*	ω_0'	*Aα*	Δ*t*
o Piscium	1h 38m 24s.36	38m 23s.01	—0s.034	—0s.28	—0s.22	—1s.17
β Arietis	47 19.90	47 18.62	—0.036	—0.21	—0.17	1.15
50 Cassiopeæ U.C. . .	52 12.43	52 11.89	- - - -	+0.56	+0.43	0.98
α Arietis	1 59 42.92	59 41.74	—0.036	—0.04	—0.15	0.99
65 Ceti	2 5 59.20	5 57.87	—0.034	—0.20	—0.22	1.08
ι Cassiopeæ U.C. . . .	3 18 12.83	18 11.83	- - - -	+0.10	+0 28	—1.28

$$6\,\Delta\theta + 0.124\,a = -0^s.079.$$
$$+0.124\,\Delta\theta + 3.652\,a = -1.230.$$
$$a = -0^s.336,\ \Delta\theta = -0^s.006,\ \Delta t = -1^s.106.$$

1866, December 16.—Gf., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
η Tauri	W.	B_1—F_5 [$F_{4.5}$ lost]	$3^h\,39^m\,34^s.05$	$+0^s.077$	$+0^m\,3^s.273$	$+0^s.062$
ζ Persei	"	B_1—D_5	45 31.15	+0.062	+0 17.979	+0.054
ζ Urs. Minoris L.C.	"	D_1—E_5	48 10.05	+0.053	+0 37.410	—0.085
" " " .	E.	E_4—F_5	3 51 1.17	+0 044	—2 13 966	—0 059
γ Tauri	"	B_1—F_5	4 12 16.04	+0 026	0 0 000	+0.009
ε "	"	B_1—F_5	20 53 65	+0.040	0 0 000	+0.022
15 Draconis L. C. .	"	B_1—C_5	27 8 58	+0.063	+1 4.277	—0.041
" " " .	W.	B_1—D_4	4 28 59.92	+0.066	—0 46.970	—0 045

$T = 4^h$ $\theta = -1^s.10$ $\rho = 0^s.000$ $c = +0^s.033$.

Star.	t	α	Cc	α_0'	$A\alpha$	Δt
η Tauri	$3^h\,39^m\,37^s\,39$	$39^m\,36^s.18$	$-0^s.036$	$-0^s.14$	$-0^s.15$	$-1^s.09$
ζ Persei	45 49.18	45 48.02	—0.039	—0.10	—0.11	1.09
ζ Ursæ Minoris L.C. .	3 48 47.26	48 44.78	- - - -	—1.38	—1.34	1.14
γ Tauri	4 12 16.05	12 14 81	+0.034	—0 10	—0 19	1 02
ε "	20 53.67	20 52.41	+0.035	—0 13	—0 17	1.05
15 Draconis L.C. . . .	4 48 12.86	28 11.12	- - - -	—0.63	—0 85	—0.88

$$6\,\Delta\theta + 8\,296\,a = -2^s.485.$$
$$+8\,296\,\Delta\theta + 22\,690\,a = -7.249.$$
$$a = -0^s\,340,\ \Delta\theta + 0^s.056,\ \Delta t = -1^s.044.$$

VI.

OBSERVATIONS AT CALAIS.

The Hardy clock was used at this station, and the transit-instrument No. 8, which had been employed with the same reticule at Mobile in 1858, and Eufaula in 1860. The cumbrous structure of the clock gave much trouble to the observers, which was increased by a couple of accidents. Some of the teeth of the escapement were bent, in the transportation or otherwise, and the performance was unsatisfactory. Still the most serious difficulties were obviated by the care and zeal of all the members of the party; and although the time-determinations could not always be all that they desired, and especially those on December 12 seem affected by some unexplained source of error, there is small doubt that had the operations continued for another week these sources of discordance would have been removed. The determinations on different dates are much more accordant than might have been expected under the circumstances, and the probable error of the final result is small.

The thread-intervals are derived from twenty-two complete transits, and the values deduced from these observations have been combined with those found for the same instrument and reticule from transits at Mobile in 1858, and Eufaula in 1860. These are given in the following table, in which the intervals previously found are reduced to the scale corresponding to the Calais focus.

EQUATORIAL THREAD-INTERVALS OF TRANSIT No. 8.

	Mobile, 1858.	Eufaula, 1860.	Calais, 1866.	Adopted.
B_1	+37ˢ.925	+37ˢ.926	+37ˢ.976	+37ˢ.942
B_2	35 194	35.205	35.214	35.204
B_3	32.664	32.665	32.611	32.647
B_4	29.959	29.933	29 986	29.069
B_5	27.214	27.215	27 183	27.204
C_1	21.728	21.742	21.810	21.760
C_2	19.069	19 056	19.047	19.057
C_3	16.286	16.284	16.271	16.280
C_4	13.644	13.619	13 583	13.615
C_5	10.950	10.930	10.948	10.943
D_1	5.546	5 562	5 528	5.545
D_2	+2.737	+2.747	+2.706	+2.130
D_3	—0.065	—0.060	—0.060	—0.062
D_4	2.794	2.796	2.641	2.740
D_5	5 527	5.532	5.555	5.538
E_1	10 922	10.909	10.936	10.922
E_2	13.600	13.594	13.663	13.619
E_3	16.371	16.378	16.376	16.375
E_4	19.032	19.035	19.013	19 027
E_5	21 700	21.686	21.679	21.688
F_1	27.137	27.113	27.115	27.122
F_2	29.871	29.870	29 850	29.864
F_3	32.591	32.612	32.626	32.610
F_4	35 305	35.315	35.321	35.314
F_5	—37.999	—38.014	—38.038	—38.017

The correction for inequality of pivots, as deduced from the Calais observations, is $+0^s.022$; the pivot at the perforated end of the axis being the smaller. The mean of the results from five previous campaigns having been found to be $+0^s.027$, and no individual determination differing from this amount by so much as $0^s.002$, the value $+0^s.025$ has been employed in the reductions.

The observations here given were made by Mr. Boutelle on December 12, 13, 14, 15, and by Mr. Chandler December 11, 16, and 18. The correction for personal equation will be applied when deducing the resultant longitudes, and is not needed here, since no group contains observations by both gentlemen. It will be seen that the transit of only a single star could be obtained before or after exchanges on the 16th, one of these being moreover a circumpolar, and that the obstacles in the way of good determinations of time were quite serious on other nights, when exchanges were made with Newfoundland. On those dates when the Calais sky was clearest, the weather at Newfoundland seems to have been unfavorable. It is to be regretted that opportunities did not continue for exchange of signals on at least two more dates, as desired by Mr. Boutelle, and the accordance of longitudes resulting from observations obtained under such unfavorable circumstances is in the highest degree honorable to the gentlemen concerned.

1866, December 11.—C., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
α Pegasi	E.	D_1—F_5 [C_3 D_4 lost]	$23^h\ 0^m\ 56^s.69$	$-0^s.18$	$+0^m\ 0^s.61$	$-0^s.18$
o Cephei U.C. . .	"	E_1—F_5 [F_4 lost]	15 0.19	—0.19	+1 0.47	—0.50
" " . .	W.	E_3—F_5	17 12.56	—0.23	—1 11.53	—0 59
θ Piscium . . .	"	B_1—F_5	24 2.57	—0.25	0 0.00	—0.21
γ Cephei U.C. . .	"	B_2—C_5	34 59.88	—0.25	+1 47.89	—1.01
" " . . .	E.	B_1—C_5	38 33.75	—0.21	—1 47.89	—0.86
Groombr. 4163 U.C.	"	E_1—$_4$	50 21.95	—0.20	+0 53.32	—0.67
" " " .	W.	E_1—F_5	23 52 47.72	—0.24	—1 27.00	—0.80

$T = 0^h$ $\theta = -2^m\ 49^s.00$ $\rho = -0^s.080$ $c = -0^s.090$.

Star.	t	a	Cc	ω_0'	Aa	Δt
α Pegasi	$23^h\ 0^m\ 57^s\ 12$	$58^m\ 8^s.02$	$-0^s.09$	$+0^s.53$	$-0^s.15$	$-2^m\ 49^s.12$
o Cephei U.C. . .	16 0.30	13 11.77	- - - -	—0.98	+0.28	48.87
θ Piscium . . .	24 2.36	21 13.13	+0.09	+0.64	—0.18	49.01
γ Cephei U.C. . .	36 45.88	33 57.53	- - - -	—2.32	+0.67	49.04
Groombr. 4163 L.C.	23 51 14.76	48 26.11	- - - -	—1.70	+0.49	—2 49.15

$$5\,\Delta\theta - 3.831\,a = +0^s.920$$
$$-3.831\,\Delta\theta + 9.906\,a = -2.704$$
$$a = -0^s.289,\ \Delta\theta = -0^s.037,\ \Delta t = +2^m\ 49^s.037.$$

1866, December 11.—C., Obs.

Star.	Lamp.	Threads.	*M*	b_0	*R*	Bb_0+k
ε Bootis	W.	$\left\{\begin{matrix} B_{1\,2\,3}\ C_{1\,2\,3}\ D_{1\,2\,3} \\ E_{1\,2\,3}\ F_{2\,3} \end{matrix}\right\}$	14h 41m 57s.21	—0s.14	+0m 2s.19	—0s.17
β Urs. Minoris U.C.	"	B_1—C_3	52 19.36	—0.14	+1 32.70	—0.53
" " "	E.	B_1—C_3	14 55 23.67	—0.10	—1 32.70	—0.39
γ² Urs. Minoris U.C.	"	E_1—F_3	15 22 22.11	—0.10	+1 20.47	—0.34
" " "	W.	F_3—E_3	25 34.77	—0.14	—1 51.71	—0.46
α Coronæ	"	$B_1\ C_{1\,2}\ D_3$	31 32.30	—0.13	+0 19.80	—0.16
α Serpentis . . .	"	E_1—F_3	15 40 57.10	—0.12	—0 24.63	—0.12

$T = 15^h$ $\theta = -2^m\ 50^s.00$ $\rho = -0^s.062$ $c = -0^s.090$.

Star.	*t*	*a*	*Cc*	a_0'	*Aa*	Δt
ε Bootis	14h 41m 59s.23	39m 9s.12	+0s.10	—0s.03	—0s.10	—2m 49s.93
β Ursæ Minoris U.C. .	14 53 51.06	51 1.57	- - - -	+0.51	+0.54	50.03
γ² " " " . .	15 23 42.45	20 52.50	- - - -	+0.08	+0.43	50.35
α Coronæ	31 51.94	29 1.70	+0.10	—0.11	—0 10	50.01
α Serpentis	15 40 32.35	37 41.60	+0.09	—0.62	—0.18	—2 50.44

$$5\ \Delta\theta - 2.054\ a = -0^s.170.$$
$$-2.054\ \Delta\theta + 6.363\ a = -1.511.$$
$$a = -0^s.287,\ \Delta\theta = -0^s.151,\ \Delta t = -2^m\ 50^s.151.$$

1866, December 12.—B., Obs.

Star.	Lamp.	Threads.	*M*	b_0	*R*	Bb_0+k
β Arietis	W.	B_1—F_3	1h 50m 10s.06	—0s.18	0m 0s 00	—0s.19
α "	"	B_1—F_3	2 2 33.07	—0.19	0 0.00	—0.21
β Urs. Minoris L.C.	E.	C_1—E_3	2 53 52.03	—0 20	+0 0.01	+0 43
ζ Arietis	"	B_1—F_3	3 10 12.67	—0.19	—0 5.14	—0.21
α Persei	"	B_1—F_3	3 17 42.95	—0.19	0 0.00	—0.31
α Camelop. U.C. .	"	B_1—F_3 [E_3F_1 lost]	4 43 50.03	—0.21	—0 5 24	—0.52
ι Aurigæ . . .	"	D_1—D_3	4 51 11.76	—0.21	—0 0.02	—0.26

$T = 3^h$ $\theta = -2^m\ 50^s.50$ $\rho = -0^s.065$ $c = -0^s.080$.

Star.	*t*	*a*	*Cc*	a_0'	*Aa*	Δt
β Arietis	1h 50m 9s.87	47m 18s.66	+0s.08	—0s.71	—0s.02	—2m 51s.19
α "	2 2 32.86	59 41.77	+0.08	—0.57	—0.02	51.05
β Ursæ Minoris U.C. .	2 53 52.47	51 1.64	+0 30	+0.04	—0.14	50.40
ζ Arietis	3 10 7.32	7 16.77	—0.08	—0.12	—0.02	50.60
α Persei	3 17 42.64	14 52.37	—0.12	+0.13	+0.01	50.38
α Camelop. U.C. . .	4 43 44.27	40 54.09	—0.20	+0.17	+0.04	50.37
ι Aurigæ	4 51 11.48	48 21.59	—0.09	+0.58	—0.01	—2 49.91

$$7\ \Delta\theta + 3.853\ a = -0^s\ 560$$
$$+ 3.853\ \Delta\theta + 12.217\ a = -.0757$$
$$a = -0^s.044,\ \Delta\theta = -0^s.056,\ \Delta t = -2^m\ 50^s.556.$$

1866, December 12.—B., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
α Canis Minoris . . .	W.	B_1—F_5	$7^h\,35^m\,12^s.03$	—$0^s.34$	$0^m\,0^s.00$	—$0^s.28$
β Geminorum	E.	B_1—F_5 [F_5 lost]	40 4 24	—0.28	—0 1.41	—0.32
ϵ Draconis L.C. . . .	"	B_1—F_5	7 51 24.32	—0.28	0 0 00	+0.39

$T = 7^h$ $\theta = -2^m\,51^s.00$ $\rho = -0^s.065$ $c = -0^s.080$.

Star.	t	α	Cc	α_0'	$A\alpha$	Δt
α Canis Minoris .	$7^h\,35^m\,11^s.75$	$32^m\,21^s.32$	+$0^s.08$	—$0^s.68$	—$0^s.06$	—$2^m50^s.26$
β Geminorum . .	40 2.51	37 11.35	—0.09	—0.21	—0.03	51.18
ϵ Draconis L. C. .	7 51 24.71	48 34.05	+0.23	+0.62	—0.26	—2 50.12

$$3\,\Delta\theta + 3.608\,a = +1^s.090$$
$$+3.608\,\Delta\theta + 7.484\,a = +1.002$$
$$a = -0^s.097,\ \Delta\theta = +0^s.480,\ \Delta t = -2^m\,50^s.529.$$

1866, December 13.—B., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
65 ξ^1 Ceti	W.	D_1—F_5	$2^h\,8^m\,50^s.32$	—$0^s.02$	$0^m\,0^s.00$	—$0^s.04$
ϵ Cassiopeæ U.C. . . .	"	B_1—F_5	21 5.36	—0.02	0 0.00	—0.09
5 Ursæ Minoris L. C. .	"	E_1—F_5	28 51.50	—0.01	+1 43.15	+0.08
" " " " . .	E.	E_1—F_5	33 18.41	+0.03	—1 43.15	—0.01
γ Ceti.	"	B_1—F_5	39 17.87	+0.03	0 0.00	0.00
α "	"	B_1—E_5	2 58 21.10	+0.06	—0 8.16	+0.02

$T = 3^h$ $\theta = -2^m\,52^s.80$ $\rho = -0^s.074$ $c = -0^s.060$.

Star.	t	α	Cc	α_0'	$A\alpha$	Δt
65 ξ^1 Ceti	2^h $8^m\,50^s.28$	$5^m\,57^s.89$	+$0^s.06$	—$0^s.40$	+$0^s.35$	—$2^m\,52^s.75$
ϵ Cassiopeæ U.C. .	21 5.27	18 11.91	+0.15	—0.47	—0.54	52.73
5 Ursæ Minoris L.C.	30 34.99	27 44.36	- - - -	+2.13	+2.08	52.75
γ Ceti	39 17.87	36 25 48	—0.06	+0.33	+0.39	52.86
α ".	2 28 12.96	55 20.63	—0.06	+0.41	+0.39	—2 52.78

$$5\,\Delta\theta + 4.612\,a = +2^s.800.$$
$$+4.612\,\Delta\theta + 15.082\,a = +8.840.$$
$$a = +0^s.578,\ \Delta\theta = +0^s.027,\ \Delta t = -2^m\,52^s.773.$$

1866, December 14.—B., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
β Arietis . .	W.	$B_1—F_5$	1h 50m 12s.44	+0s.13	0m 0s.00	+0s.11
α " . . .	"	$B_1—F_5$ [C_4 lost]	2 2 36.31	+0.14	—0 0.62	+0.12
65 ξ¹ Ceti . .	"	$B_1—D_3$	8 32.30	+0.14	+0 19.65	+0.09
ε Cassiopeæ U.C.	"	$B_1—F_5$	21 5.66	+0.15	0 0.00	+0.31
γ Ceti . . .	E.	$B_1—C_3E_4F_5$ [F_3 lost]	39 22.72	+0.21	—0 3.31	+0.13
α "	"	$B_1—F_5$	2 58 14.52	+0.17	0 0.00	+0.11
α Persei . . .	"	$B_1—F_5$	3 17 46.04	+0.14	0 0.00	+0.19

$T = 3^h$. $\theta = -2^m\ 54^s.00$. $\rho = -0^s.081$. $c = -0^s.660$.

Star.	t	a	Cc	w_0'	Aa	Δt
β Arietis	1h 50m 12s.55	47m 18s.64	+0s.06	+0s.05	—0s.04	—1m 53s.91
α "	2 2 35.81	59 41.76	+0.06	—0.07	—0.04	54.03
65 ξ¹ Ceti . . .	8 52.04	5 57.88	+0.06	—0.17	—0.06	54.11
ε Cassiopeæ U.C. .	21 5.97	18 11.89	+0.15	+0.02	+0.09	54.07
γ Ceti.	39 19.54	36 25.47	—0.06	—0.16	—0.06	54.10
α "	2 58 14.63	55 20.63	—0.06	—0.06	—0.06	54.00
α Persei	3 17 46 23	14 52.36	—0.09	+0.06	+0.01	—2 53.95

$$7\,\Delta\theta + 1.767\,a = -0^s.330.$$
$$+1\,767\,\Delta\theta + 2.531\,a = -0.282.$$
$$a = -0^s.095,\ \Delta\theta = -0^s.023,\ \Delta t = -2^m\ 54^s.023.$$

1866, December 14.—B., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
μ Geminorum . .	E.	$B_1—_5F_{2—5}$	6h 17m 54s.26	+0s.10	—0m 3s.27	+0s.08
γ " . . .	W.	$B_1—F_1$ [$B_5—C_2$ lost]	32 52.26	+0.04	+0 4.92	+0.02
ε Canis Majoris .	"	$D_3—E_5$	6 56 32.48	+0.04	—0 12.83	—0.01
δ " " . .	"	$B_5—E_4$	7 5 51.31	+0.04	+0 3.63	—0.01
δ Geminorum . .	"	$B_{1—5}C_{1\,4\,5}D_{2\,4}F_{1—5}$	7 15 2.51	+0.04	+0 3.61	+0.02

$T = 7^h$ $\theta = -2^m\ 54^s.30$ $\rho = -0^s.081$ $c = -0^s.060$.

Star.	t	a	Cc	w_0'	Aa	Δt
μ Geminorum . . .	6h 17m 51s.07	14m 56s.07	—0s.06	rejected	- - - -	- - - - - -
γ " . . .	32 57.20	30 2.84	+0 06	—0s.04	+0s.03	—2m 54s 37
ε Canis Majoris . .	6 56 19.64	53 25.41	+0.07	+0.14	+0.07	54.23
δ " " . . .	7 5 54.93	3 0.42	+0.07	—0.13	+0.07	54.50
δ Geminorum . . .	7 15 6.14	12 11.75	+0.06	—0.01	+0.03	—2 54.34

$$4\,\Delta\theta + 3.075\,a = -0^s.040$$
$$+3.075\,\Delta\theta + 2.747\,a = -0.007$$
$$a = +0^s.063,\ \Delta\theta = -0^s.058,\ \Delta t = -2^m\ 54^s.358$$

1866, December 15.—B., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
α Arietis	W.	B_2 C_2 D_2 E_2 F_2	2h 2m 37s.56	+0s.26	—0m 0s.03	+0s.24
ε Cassiopeæ U.C.	"	B_1—F_5	21 7.40	+0.19	0 0.00	+0.40
5 Urs. Minoris L.C.	E.	C_2—F_5	31 29.68	+0.23	—0 47.75	—0.45
γ Ceti	"	B_1—E_5	39 21.63	+0.24	0 0.00	+0.16
α "	"	B_1—F_5 [$C_{1,2}$ D_1 E_1 lost]	2 58 14.22	+0.25	+0 2.59	+0.17
48 Cephei U.C.	"	B_1—F_5 [D_2 lost]	3 6 34.46	+0.27	—0 0.01	+0.96
α Persei	W.	B_1—F_5	17 48.93	+0.25	0 0.00	+0.36
γ² Urs. Minoris U.C.	"	$B_{1,2}$ C_1—F_4	23 57.72	+0.26	—0 8.41	+0.34
δ Persei	"	B_1—F_5	36 25.82	+0.26	0 0.00	+0.36
η Tauri	E.	B_1—F_5	3 42 32.30	+0.30	0 0.00	+0.28

$T = 3^h$ $\theta = -2^m\ 56^s.20$ $\rho = 0^s.087$ $c = -0^s.060$.

Star.	t	α	Cc	α_0'	$A\alpha$	Δt
α Arietis	2h 2m 37s.77	59m 41s.75	+0s.06	+0s.15	—0s.06	—2m 56s.00
ε Cassiopeæ U.C.	21 7.80	18 11.86	+0.15	+0.35	+0.12	55.98
5 Urs. Minoris L.C.	30 41.48	27 44.46	+0.25	—0.61	—0.48	56.33
γ Ceti	39 21.79	36 25.47	—0.06	—0.24	—0.09	56.35
α "	2 58 16.98	55 20.63	—0.06	—0.21	—0.09	56.32
48 Cephei U.C.	3 6 35.41	3 39.64	—0.27	+0.17	+0.32	56 35
α Persei	17 49.29	14 52.36	+0.09	rejected	- - - -	- - - - - -
γ² Urs. Minoris L.C.	23 48.97	20 52.65	—0.20	—0.29	—0.39	56.10
δ Persei	36 26.18	33 29.75	+0.09	—0.11	+0.01	56.32
η Tauri	3 42 32.58	39 36.18	—0.06	—0.20	—0.05	—2 56.35

$$9\,\Delta\theta + 5.275\,a = -0^s.990$$
$$+5.275\,\Delta\theta + 29.338\,a = -4.092$$
$$a = -0^s.134,\ \Delta\theta = -0^s.032,\ \Delta t = -2^m\ 56^s.232.$$

1866, December 16.—C., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
ο Cephei U.C.	E.	B_5—C_2 D_3—E_1	23h 16m 26s.26	+0s.15	—0m 16s.74	+0s.32
ε Aurigæ	"	B_1—F_5	4 51 19.92	+0.11	0 0.00	+0.10

$T = 3^h$ $\theta = -2^m\ 58^s.40$ $\rho = -0^s.087$ $c = -0^s.060$.

Star.	t	α	Cc	α_0'	$A\alpha$	Δt
ο Cephei U.C.	23h 16m 9s.84	13m 11s.53	—0s.16	—0s.39	—0s.39	—2m 58s.40
ε Aurigæ	4 51 20.02	48 21.63	—0.07	+0.10	+0.10	—2 58.40

$$2\,\Delta\theta - 0.730\,a = -0^s.290$$
$$-0.730\,\Delta\theta + 1.031\,a = +0.408$$
$$a = +0^s.395,\ \Delta\theta = -0^s.001,\ \Delta t = +2^m\ 58^s.401.$$

1866, December 18.—C., Obs.

Star.	Lamp.	Threads.	M	b_0	R	Bb_0+k
α Camelop. U.C. . .	E.	$\left\{\begin{matrix} B_1—F_5\,[C_{345} \\ D_2 \text{ lost}] \end{matrix}\right\}$	$4^h\,43^m\,51^s.78$	$+0^s.04$	$+0^m\,5^s.12$	$+0^s.05$
ι Aurigæ	"	$B_1—F_5$	51 24.16	+0.06	0 0.00	+0.05
ε Ursæ Minoris L.C.	"	$B_5—C_3$	4 59 28.97	+0.07	+3 1.47	—0.21
" " "	W.	$B_1—C_5$	5 5 30.99	+0.04	—3 1.47	—0.03
α Aurigæ	"	$D_1—F_5$	10 19.96	+0.05	—0 23.42	+0.05
β Tauri	"	$B_1—C_5$ [C_1 lost]	20 39.22	+0.05	+0 18.11	+0.03
δ Orionis	"	$B_1—F_5$	28 16.37	+0.05	0 0.00	+0.02
ε Orionis	"	$B_1—F_5$ [E_5 lost]	32 30.73	+0.05	+0 0.90	+0.01
ψ′ Draconis L. C. .	"	$E_1—F_5$	45 54.84	+0.05	+1 20.07	—0.03
" " " . .	E.	$E_2—F_5$	48 40.23	+0.09	—1 24.99	—0.09
α Arionis	"	$B_1—F_3$	5 51 1.82	+0.09	0 0.00	+0.05

$T = 5^h \quad \theta = -3^m\,3^s.00 \quad \rho = -0^s.088 \quad c = -0^s.050.$

Star.	t	a	Cc	ω_0'	Aa	Δt
α Camelop. L. C. . .	$4^h\,43^m\,56^s.95$	$40^m\,54^s.12$	$-0^s.12$	$+0^s.03$	$-0^s.52$	$-3^m\,2^s.45$
ι Aurigæ	4 51 24.21	48 21.64	—0.06	+0.36	+0.15	2.79
ε Ursæ Minoris L.C.	5 2 39 86	59 31.22	- - - -	+4.36	+3.50	2.14
α Aurigæ	9 56.59	6 53.93	+0.07	+0.42	—0.01	2.57
β Tauri	20 57.36	17 54.56	+0.06	+0.19	+0.19	3.00
δ Orionis	28 16.39	25 13.98	+0.05	+0.68	+0.42	2.74
ε "	32 31.64	29 29.13	+0.05	+0.58	+0.43	2.85
ψ′ Draconis . . .	47 15.01	44 14.01	- - - -	+2.07	+1.73	2.66
α Orionis	5 51 1.87	47 59.54	—0.05	+0.70	+0.37	—3 2.67

$$9\,\Delta\theta + 10.537\,a = +\,9^s.390$$
$$+\,10.537\,\Delta\theta + 49.521\,a = +\,33.065$$
$$a = +\,0^s.594,\ \Delta\theta = +\,0^s.348,\ \Delta t = -\,3^m\,2^s.562.$$

VII.

LONGITUDE SIGNALS BETWEEN FOILHOMMERUM AND HEART'S CONTENT.

The method of giving and receiving the signals for longitude, between Foilhommerum and Heart's Content, was that prescribed in the programme which I had prepared before leaving home. Three series, of two sets each, were exchanged on every occasion; each set consisting of ten signals alternately positive and negative, at intervals of about five seconds, except that the fifth and eighth were preceded by pauses of ten seconds, which was also the interval between the two sets. The purpose of this arrangement was to discover whether the velocity of transmission was perceptibly affected by a longer time being allowed for the cable to recover its electrical equilibrium, and also to facilitate the identification of the individual signals. Some slight convenience in the practical details also arose from the circumstance that each set occupied one minute, and that each series consisted of ten positive and ten negative signals. Those signals were considered positive by which the platinum was put in connection with the cable and the zincode with the ground.

In receiving the signals, the observer (Mr. Dean at Newfoundland, and myself at Valencia) watched the deflections of the light-spot, while his thumb rested on the button of a delicately adjusted break-circuit key, which was pressed at the instant in which the deflection was perceived. This instant was thus recorded upon the chronograph, after a certain amount of delay, which we will call the personal error of noting, and which depended upon a considerable number of influences to be discussed hereafter. The keys by which the signals were transmitted were made by the American Telegraph Company, under the supervision of Mr. Dean, and are constructed according to the arrangement devised by Prof. Thomson for the Atlantic Telegraph, in such a manner that pressure upon one button produces a positive, and upon the other a negative signal, while no current flows at other times. To this arrangement an additional contrivance was applied by which the local circuit to the chronograph passed through the same key, and was interrupted by pressure upon either button, so that every signal transmitted through the cable was recorded upon the chronograph at the station whence it was sent.

It is thus manifest that the times of sending the signals were accurately recorded, while the times of receiving signals were recorded after an interval of time dependent on the personal error of noting, and inseparable from the time of transmission through the cable, except by some independent means of measurement. If this interval were the same for both observers, it would be eliminated entirely from the longitude and merged with the time of transmission. Otherwise it would affect the resultant longitude by one-half the difference between the personal errors of noting for the two observers. Happily it proved to be very nearly the same for Mr. Dean and myself, and also measurable; so that it has been possible to eliminate its influence from the measure of velocity, as well as from the longitude.

If now we denote the clock-times at Valencia and Newfoundland by T and T'' respectively, the corrections for reducing these to the true sidereal time by Δt and $\Delta t'$, the time required for transmission of the galvanic signals by x, and the longi-

tude by λ; and if furthermore we distinguish those quantities which pertain to Valencia signals by a subjacent 1, and similarly those belonging to Newfoundland signals by the subjacent figure 2, it is manifest that, if we include in x the personal error of noting signals, then

the signals given and recorded at Valencia at the time T_1 will be registered upon the Newfoundland record at $T_1' = T_1 + \Delta t_1 - \Delta t_1' - \lambda + x_1$

and the signals given and recorded at Newfoundland at T_2 will be registered upon the Valencia record at $T_2 = T_2' + \Delta t_2' - \Delta t_2 + \lambda + x_2$

Thus the comparison of the records of Valencia signals, at the two stations, gives

$$T_1 - T_1' = \Delta t_1' - \Delta t_1 + \lambda - x_1$$

while the comparison of the records of Newfoundland signals gives

$$T_2 - T_2' = \Delta t_2' - \Delta t_2 + \lambda + x_2$$

and consequently

$$2\lambda = (T_2 - T_2') + (T_1 - T_1') + (\Delta t_2 - \Delta t_2') + (\Delta t_1 - \Delta t_1') + (x_1 - x_2)$$

$$x_1 + x_2 = (T_2 - T_2') - (T_1 - T_1') + (\Delta t_2 - \Delta t_2') - (\Delta t_1 - \Delta t_1')$$

If we assume the personal error of noting to be the same for the two observers, and the signals to travel with equal velocity in the two directions, the term $x_1 - x_2$ will disappear from the first equation, while the second will give a measure of the sum of the transmission-times and the personal errors of noting.

From the time-determinations in Chapters IV and V we may obtain the clock-corrections as follows, for the periods in which longitude-signals were exchanged. They are first given for those epochs for which they were determined, and the interpolated values follow for intervals of five minutes during the period of the exchanges.

On November 5, a double weight is assigned to the first time-determination, on account of the much smaller value of the azimuth error; this having been largely changed by an accident (due to no carelessness of the observer) which interrupted the first series of observations.

CLOCK-CORRECTIONS AT EACH STATION.

Valencia Clock Corrections.			Newfoundland Clock Corrections.		
	Sid. time $=T$.	Δt		Sid. time $=T'$.	$\Delta t'$
1866.					
October 25,	$19^h.8$	$-8^s.447$	October 25,	$21^h.0$	$+4^s.714$
				0.0	+4.745
27,	1.7	—8.420			
			October 28,	22.0	+5.640
28,	21.0	—8.812		1.0	+5.713
30,	22.0	—9.266	November 5,	23.0	+8.326
				2.0	+8.332
November 5,	23.0	—13.034			
	"	.058	November 6,	21 0	+7.973
				23.0	+7.967
6,	23.0	—13.236			
	3.0	364	November 9,	21.0	+7.810
				1.0	+7.813
9,	20.0	—15.977			
	4.0	—16.608			
October 25,	$1^h\,50^m$	$-8^s.442$		$23^h\ 0^m$	$+4^s.735$
	1 55	.441		23 5	.736
	2 0	.441		23 10	.737
	2 5	.441		23 15	.738
October 28,	2 35	—8.864		23 40	+5.681
	2 40	.865		23 45	.683
	2 45	.865		23 50	.685
	2 50	.866		26 55	.687
	2 55	.867		0 0	.689
November 5,	3 30	—13.078		0 40	+8.329
	3 35	.079		0 45	.329
	3 40	.079		0 50	.329
	3 45	.080		0 55	.329
	3 50	.080		1 0	.329
November 6,	1 10	—13.305		22 15	+7.967
	15	.308		22 20	.967
	20	.311		22 25	.966
	25	.313		22 30	.966
November 9,	3 0	—16.529		0 10	+7.812
	5	.535		0 15	.812
	10	.542		0 20	.812
	15	.548		0 25	.813
	20	.555		0 80	.813

We are thus enabled to construct the following table, in which the times given are the means of the clock-times for each set, to the nearest minute, and the results for positive and negative signals are exhibited separately as well as together.

TRANS-ATLANTIC LONGITUDE AND TRANSMISSION-TIME.

1866, October 25.

		Series I.		Series II.		Series III.	
Valencia signals.	T_1	$1^h 49^m 01^s$	$1^h 50^m 7^s$	$1^h 55^m 27^s$	$1^h 56^m 32^s$	$2^h 1^m 48^s$	$2^h 2^m 53^s$
	T_1'	22 56 52	22 57 58	23 3 18	23 4 23	23 9 40	23 10 45
	pcs.	2 52 9.02	2 52 8.99	2 52 9.03	2 52 9.05	2 52 9.08	2 52 9.09
	neg	8.90	9.05	8.95	9.06	9.13	9.10
	all.	8.96	9.02	8.99	9.05	9.11	9.10
	$\Delta t_1 - \Delta t_1'$	—13.176	—13.176	—13.177	—13.177	—13.178	—13.178
Newfoundland sigs.	T_2	1 52 9	1 53 15	1 58 41	1 59 6	2 5 11	2 6 17
	T_2'	23 0 0	23 1 5	23 6 30	23 7 36	23 13 1	23 14 6
	pos.	2 52 10.26	2 52 10.25	2 52 10.25	2 52 10.27	2 52 10.26	2 52 10.29
	neg.	10.26	10.26	10.29	10.28	10.25	10.32
	all.	10.26	10.26	10.27	10 28	10.25	10.30
	$\Delta t_2 - \Delta t_2'$	—13.176	—13.176	—13.177	—13.177	—13.178	—13.178
Sum of intervals.	Corr.	—26.352	—26.352	—26.354	—26.354	—26.356	—26.356
	pos.	5 44 19.28	5 44 19.24	5 44 19.28	5 44 19.32	5 44 19.34	5 44 19.38
	neg.	19.16	19.31	19.24	19.34	19.38	19 42
	all.	19.22	19.28	19.26	19.33	19.36	19.40
x	pos.	0.62	0.63	0 61	0.61	0.59	0.60
	neg.	0.68	0.60	0.67	0.61	0.56	0.61
	all.	0.65	0.62	0.64	0.61	0.57	0.60
	λ	$2^h 51^m 56^s.449$.		$2^h 51^m 56^s.470$.		$2^h 51^m 56^s.512$.	

1866, October 28.

Valencia signals.	T_1	$2^h 33^m 30^s$	$2^h 34^m 35^s$	$2^h 39^m 35^s$	$2^h 40^m 40^s$	$2^h 52^m 37^s$	$2^h 53^m 42^s$
	T_1'	23 41 20	23 42 25	23 47 25	23 48 30	0 0 27	0 1 32
	pos.	2 52 10.34	2 52 10.36	2 52 10.40	2 52 10.39	2 52 10.43	2 52 10.42
	neg.	10.39	10.36	10.40	10.41	10.42	10.42
	all.	10.37	10.36	10.40	10.40	10.42	10.42
	$\Delta t_1 - \Delta t_1'$	—14.546	—14.547	—14.549	—14.550	—14.556	—14.556
Newfoundland sigs.	T_2	2 36 37	2 37 43	2 42 38	2 43 43	2 56 40	2 56 45
	T_2'	23 44 25	23 45 31	23 50 26	23 51 31	0 3 28	0 4 33
	pos.	2 52 11.78	2 52 11.79	2 52 11.65	2 52 11.67	2 52 11.68	2 52 11.66
	neg.	11.69	11.68	11.64	11.72	11.57	11.69
	all.	11.73	11 74	11.64	11.69	11.62	11.67
	$\Delta t_2 - \Delta t_2'$	—14.548	—14.548	—14.551	—14.551	—14.557	—14.558
Sum of intervals.	Corr.	—29.094	—29.095	—29.100	—29.101	—29.113	—29.114
	pos.	5 44 22.12	5 44 22.15	5 44 22.05	5 44 22.06	5 44 22.11	5 44 22.08
	neg.	22.08	22.04	22.04	22.13	21.97	22.11
	all.	22.10	22.10	22.04	22.09	22.04	22.09
x	pos.	0.72	0.72	0.62	0.64	0.62	0.62
	neg.	0.65	0.66	0.62	0.66	0.58	0.64
	all.	0.68	0.69	0.62	0.65	0.60	0.63
	λ	$2^h 51^m 56^s.502$.		$2^h 51^m 56^s.482$.		$2^h 51^m 56^s.476$.	

1866, November 5.

		Series I.		Series II.		Series III.	
Valencia signals.	T_1	$3^h 32^m 35^s$	$3^h 33^m 41^s$	$3^h 39^m 22^s$	$3^h 40^m 27^s$	$3^h 46^m 0^s$	$3^h 47^m 0^s$
	T_1'	0 40 18	0 41 23	0 47 5	0 48 10	0 53 42	0 54 42
	pos.	2 52 17.25	2 52 17.30	2 52 17.32	2 52 17.31	2 52 17.25	2 52 17.27
	neg.	17.25	17.29	17.30	17.31	17.26	17.25
	all.	17.25	17.30	17.31	17.31	17.26	17.26
	$\Delta t_1 - \Delta t_1'$	—21.407	—21.407	—21.408	—21.408	—21.409	—21.409
Newfoundland sigs.	T_2	3 35 45	3 36 50	3 42 46	3 43 51	3 49 19	3 50 24
	T_2'	0 43 27	0 44 32	0 50 28	0 51 33	0 57 1	0 58 6
	pos.	2 52 18.42	2 52 18.47	2 52 18.44	2 52 18.47	2 52 18.46	2 52 18.47
	neg.	18.43	18.48	18.43	18.55	18.46	18.47
	all.	18.42	18.47	18.43	18.51	18.46	18.47
	$\Delta t_2 - \Delta t_2'$	—21.408	—21.408	—21.408	—21.408	—21.409	—21.409
Sum of intervals.	Corr.	—42.815	—42.815	—42.816	—42.816	—42.818	—42.818
	pos.	5 44 35.67	5 44 35.77	5 44 35.76	5 44 35.78	5 44 35.71	5 44 35.74
	neg.	35.68	35.77	35.73	35.86	35.72	35.72
	all.	35.67	35.77	35.74	35.82	35.72	35.73
x	pos.	0.57	0.58	0.56	0.58	0.60	0.60
	neg.	0.59	0.60	0.56	0.62	0.60	0.61
	all.	0 58	0.59	0.56	0.60	0.60	0.60
	λ	$2^h 51^m 56^s.428$.		$2^h 51^m 56^s.482$.		$2^h 51^m 56^s.454$.	

1866, November 6.

Valencia signals.	T_1	$1^h 9^m 10^s$	$1^h 10^m 15^s$			$1^h 21^m 20^s$	$1^h 22^m 17^s$
	T_1'	22 16 53	22 17 58			22 29 3	22 30 0
	pos	2 52 17.19	2 52 17.20			2 52 17.18	2 52 17.20
	neg.	17.22	17.18			17.30	17.18
	all.	17.20	17.19			17.24	17.19
	$\Delta t_1 - \Delta t_1'$	—21.272	—21.272			—21.277	—21.278
Newfoundland sigs.	T_2	1 12 25	1 13 30			1 24 2	1 25 32
	T_2'	22 20 6	22 21 11			22 32 86	22 33 14
	pos.	2 52 18.32	2 52 18.31			2 52 18.25	2 52 18.33
	neg.	18.30	18.33			18.29	18.33
	all.	18.31	18.32			18.27	18.33
	$\Delta t_2 - \Delta t_2'$	—21.273	—21.274			—21.279	—21.279
Sum of intervals.	Corr.	—42.545	—42.546			—42.556	—42.557
	pos.	5 44 35.51	5 44 35.51			5 44 35.43	5 44 35.53
	neg.	35.52	35.51			35.59	35.51
	all.	35.51	35.51			35.51	35.52
x	pos.	0.56	0.55			0.54	0.56
	neg.	0.54	0.57			0.50	0.58
	all.	0.55	0.56			0.52	0.57
	λ	$2^h 51^m 56^s.482$.				$2^h 51^m 56^s.480$.	

1866, November 9.

		Series I.		Series II.		Series III.	
Valencia signals.	T_1	$3^h\ 0^m\ 0^s$	$3^h\ 1^m\ 6^s$	$3^h 10^m 12^s$	$3^h 11^m 17^s$	$3^h 16^m 23^s$	$3^h 17^m 28^s$
	T_1'	0 7 40	0 8 45	0 17 52	0 18 56	0 24 3	0 25 8
	pos.	2 52 20.25	2 52 20.27	2 52 20.30	2 52 20.26	2 52 20.27	2 52 20.29
	neg.	20.27	20.30	20.28	20.26	20.29	20.26
	all.	20.26	20.28	20.29	20.26	20.28	20.28
	$\Delta t_1 - \Delta t_1'$	—24.341	—24.342	—24.354	—24.355	—24.363	—24.364
Newfoundland sigs.	T_2'	3 3 6	3 4 11	3 13 13	3 14 18	3 19 44	3 20 50
	T_2	0 10 44	0 11 49	0 20 51	0 21 56	0 27 25	0 28 28
	pos.	2 52 21.32	2 52 21.34	2 52 21.34	2 52 21.39	2 52 21.35	2 52 21.41
	neg.	21.34	21.33	21.36	21.37	21.37	21.36
	all.	21.33	21.33	21.35	21.38	21.36	21.39
	$\Delta t_2 - \Delta t_2'$	—24.345	—24.346	—24.358	—24.360	—24.367	—24.369
	Corr.	—48.686	—48.688	—48.712	—48.715	—48.730	—48.733
Sum of intervals.	pos.	5 44 41.57	5 44 41.61	5 44 41.64	5 44 41.65	5 44 41.62	5 44 41.70
	neg.	41.61	41.63	41.64	41.63	41.66	41.62
	all.	41.59	41.62	41.64	41.64	41.64	41.66
x	pos.	0.53	0.53	0.52	0.57	0.54	0.56
	neg.	0.53	0.51	0.54	0.55	0.54	0.55
	all.	0.53	0.52	0.53	0.56	0.54	0.56
	λ	$2^h\ 51^m\ 56^s.459$.		$2^h\ 51^m\ 56^s.463$.		$2^h\ 51^m\ 56^s.459$.	

It is manifest that $\Delta t - \Delta t'$ was in no instance variable during the telegraphic exchanges, so that no correction is needed for the deduced values of x on account of difference of clock-rates; and there is every reason to believe, both from theoretical considerations and from special experiment, that the velocity is the same for eastward and for westward signals, and that the resultant λ is consequently subject to no correction depending upon the clocks.

The resultant values of the longitude are thus found to be

1866, October	25,	$2^h\ 51^m\ 56^s.477$
	28,	56.487
November	5,	56.455
	6,	56.481
	9,	56.460

subject, however, in every case to a correction for personal equation in determining the time.

The mean interval between the moments of giving the signals and of their record upon the chronograph sheet is similarly found to have been

October	25,	$0^s.62 \pm 0^s.008$
	28,	0.64 .010
November	5,	0.59 .004
	6,	0.55 .007
	9,	0.54 .005

in which the quantities appended are the probable errors of the respective deter-

minations as deduced from the total results of the several sets, there being six sets for each determination except that of November 6.

On the 25th October the cable of 1865 was employed, one-half the circuit being formed by the earth; a battery of ten cells was used at each station, and "condensers" were interpolated between the battery and the cable. On the 28th October the connections were the same, in every respect, as on the 25th; but on the three other days the two cables were joined so as to form a complete metallic circuit, the number of elements employed being—

November 5,	3	at Valencia,	3	at Newfoundland.
6,	3	"	10	"
9,	4	"	10	"

VIII.

LONGITUDE SIGNALS BETWEEN HEART'S CONTENT AND CALAIS.

Clock-signals were exchanged between these two stations on four nights, upon only two of which the clock-errors at Calais could be determined either immediately before, or soon after, the exchange, one of these two nights being the same on which the clock occasioned so much trouble. It is a source of regret also that the signals were not exchanged according to the rule laid down in the programme, which prescribed that the Calais clock should be put into the circuit several times, for not more than half a minute at each time, while the time-scale was graduated for both chronographs by the Heart's Content clock only. In this way it would not have been difficult to obtain both records on the sheets at both stations with the ordinary connections, and without the necessity of continual adjustments of the relay magnets, and the results would have been more satisfactory in other respects.

On the 11th and 12th December, only the first and last signals of the Heart's Content clock in each minute can be deciphered, but these are legible by reason of the omission of the second-marks corresponding to the beginning of the minute. For the other two nights this difficulty does not exist. The means of the records are appended for the two stations separately. Upon the first two dates the individual measures from the Calais registers, although numbering but two in each minute, were derived from consecutive minutes.

NEWFOUNDLAND SIGNALS.

Date.	No.	H. C. clock-time.	Calais clock-time.
1866, December 11.	10	$6^h\ 46^m\ 0^s.0$	$5^h\ 53^m\ 13^s.912$
	10	7 8 0.0	6 15 13.908
December 12.	10	6 21 0.0	5 28 14.828
	16	7 29 30.0	6 36 44.813
	12	7 57 30.0	7 4 44.838
December 14.	38	5 43 40.0	4 50 56.462
	40	44 20.0	51 36.461
	39	45 0.0	52 16.453
	39	45 40.0	52 56.451
	40	5 46 20.0	4 53 36.455

Date.	No.	H. C. clock-time.	Calais clock-time.
1866, December 14.	39	$5^h\ 47^m\ 0^s.0$	$4^h\ 54^m\ 16^s.450$
	59	5 47 50.0	4 55 6.446
	56	5 54 17.0	5 1 33.457
	58	55 25.0	2 41.454
	54	56 23.0	3 39.451
	57	5 57 45.0	5 5 1.444
	38	6 5 4.0	5 12 20.459
	56	5 54.0	13 10.445
	55	6 54.0	14 10.440
	59	7 54.0	15 10.463
	59	8 54.0	16 10.464
	59	6 9 44.0	5 17 0.457
December 16.	59	2 26 20.0	1 33 39.714
	59	27 20.0	34 39.709
	59	28 20.0	35 39.711
	52	2 29 20.0	1 36 39.734
	59	2 37 4.0	1 44 23.689
	58	38 3.0	45 22.691
	59	39 4.0	46 23.701
	47	2 40 0.0	1 47 19.700
	53	2 48 22.0	1 55 41.704
	56	49 23.0	56 42.707
	56	50 23.0	57 42.709
	57	2 51 23.0	1 58 42.719

Calais Signals.

Date.	No.	Calais clock-time.	H. C. clock-time.
1866, December 11.	59	$5^h\ 43^m\ 26^s.0$	$6^h\ 36^m\ 12^s.598$
	59	44 26.0	37 12.594
	58	45 25.0	38 11.594
	56	46 25.0	39 11.578
	59	5 47 26.0	6 40 12.562
	58	5 59 15.0	6 52 1.586
	80	6 0 26.0	53 12.578
December 12.	56	5 32 25.0	6 25 10.797
	50	33 23.0	26 8.793
	56	34 25.0	27 10.803
	61	5 35 30.0	28 15.818
	55	6 28 5.0	7 20 50.783
	56	29 5.0	21 50.786
	58	30 5.0	22 50.773
	59	31 7.0	23 52.761
	60	6 32 7.0	7 24 52.770
December 14.	59	4 56 36.0	5 49 20.113
	59	57 36.0	50 20.103
	59	58 36.0	51 20.102
	58	59 36.0	52 20.102
	57	5 0 36.0	53 20.099

Date.	No.	Calais clock-time.	H. C. clock-time.
1866, December 14.	57	$5^h\ 6^m\ 50^s.0$	$5^h\ 59^m\ 34^s.096$
	57	7 50.0	6 0 34.092
	41	8 48.0	1 32.092
	18	5 9 46.0	6 2 30.091
	36	5 18 10.0	6 10 54.086
	62	5 21 6.0	6 13 50.143
	34	5 22 31.0	6 15 15.168
December 16.	56	1 38 32.0	2 31 12.901
	39	39 28.0	32 8.926
	59	40 32.0	33 12.902
	59	41 32.0	34 12.905
	59	1 42 32.0	2 35 12.897
	42	1 49 33.0	2 42 13.832
	55	50 34.0	43 14.843
	55	51 34.0	44 14.869
	59	52 34.0	45 14.877
	58	1 53 34.0	2 46 14.881
	57	2 0 39.0	2 53 19.839
	47	1 35.0	54 15.843
	56	2 39.0	55 19.830
	52	2 3 38.0	2 56 18.847

From the reductions of Chapters V and VI we may deduce the following determinations of the clock-corrections at the two stations:—

1866.	Heart's Content clock-corrections. Sid. clock time, T'	$\Delta t'$	Calais clock-corrections. Sid. clock time, T''	$\Delta t''$
December 11	$6^h\ 35^m$	$+2^s.080$	$5^h\ 40^m$	$-2^m\ 49^s.457$
	40	.079	45	.463
	45	.078	50	.469
	50	.077	55	.475
	55	.075	6 0	.482
	7 0	.074	5	.488
	5	.073	10	.494
	7 10	+2.072	6 15	−2 49.501
December 12	6 20	+1.538	5 25	−2 50.535
	25	.536	30	.534
	6 30	.535	5 35	.533
	7 20	.518	6 25	.526
	25	.516	30	.525
	30	.515	35	.524
	7 55	.505	7 0	.520
	8 0	+1.504	7 5	−2 50.519
December 14	5 40	−0.149	4 50	−2 54.204
	45	.152	55	.197
	50	.156	5 0	.190
	55	.160	5	.183
	6 0	.163	10	.176
	5	.167	15	.169
	10	.170	20	.162
	6 15	−0.174	5 25	−2 54.155

	Heart's Content clock-corrections.		Calais clock-corrections.	
	T'	$\Delta t'$	T''	$\Delta t''$
December 16.	2h 25m	—1s.092	1h 30	—2 58.266
	30	.090	35	.274
	35	.087	40	.281
	40	.085	45	.288
	45	.082	50	.295
	50	.080	1 55	.303
	2 55	.077	2 0	.311
	3 0	— .075	2 5	—2 58.318

We have thus from the Heart's Content signals, recorded at Calais—

Date.	T'	No. signals.	Difference of clocks.	$\Delta t'-\Delta t''$	$\lambda-x'$
1866, December 11.	6h 46m 0s	10	0h 52m 46s.09	+2m 51s.55	0h 55m 37s.64
	7 8 0	10	46.09	51.57	37.66
December 12.	6 21 0	10	0 52 45.17	+2 52.07	0 55 37.24
	7 29 30	16	45.19	52.04	37.23
	7 57 30	12	45.16	52.02	37.18
December 14.	5 45 40	294	0 52 43 56	+2 54.04	0 55 37.59
	5 56 0	225	43.55	54.02	37.57
	6 7 30	326	43.55	54.00	37.55
December 16.	2 27 50	229	0 52 40.28	+2 57.18	0 55 37.46
	2 38 30	223	40.30	57.20	37.50
	2 50 0	222	40.29	57.22	37.51

and from the Calais signals, recorded at Heart's Content—

Date.	T'	No. signals.	Difference of clocks.	$\Delta t'-\Delta t''$	$\lambda-x$
1866, December 11.	6h 38m 12s	291	0h 52m 46s.59	+2m 51s.54	0h 55m 38s.13
	6 52 36	138	46.58	+2 51.55	38.13
December 12.	6 26 40	223	0 52 45.80	+2 52.07	0 55 37.87
	7 22 50	288	45.78	+2 52.04	37.82
December 14.	5 51 20	292	0 52 44.10	+2 54.03	0 55 38.13
	6 0 40	177	44.09	54.01	38.10
	6 10 54	36	44.09	53.99	38.08
	6 14 25	96	44.16	+2 53.98	38.14
December 16.	2 33 12	272	0 52 40.91	+2 57.19	0 55 38.10
	2 44 15	269	40.86	57.21	38.07
	2 55 30	212	40.84	+2 57.23	38.07

From these we find the several values of the longitude and time of transmission—

1866.	λ	x
December 11.	0h 55m 37s.89	0s.24
12.	37.53	0.31
14.	37.84	0.27
16.	37.78	0.28

the longitude results requiring, however, a correction for personal equation.

IX.

PERSONAL ERROR IN NOTING SIGNALS.

Since the signals sent through the telegraphic cable were recorded upon the chronograph automatically at the transmitting station, but at the receiving station through the mediation of an observer, who noted the deflection of the light-spot from the galvanometer by sending a second telegraphic signal to his own chronograph, it will be seen that the interval x, which elapses between the giving of a signal at one station and its chronographic record at the other, may be conveniently divided into four different parts, viz., the time requisite

1. For the signal to arrive at the other station;
2. For the magnet of the galvanometer to be moved through an arc sufficient to be readily perceived;
3. For the observer to take cognizance of the deflection, and give his signal upon the break-circuit key;
4. For this observation-signal to be recorded upon the chronograph.

Each of these four parts comprises the time, appreciable or otherwise, consumed in more than one distinct process; yet this division suffices for all our purposes. If these several intervals be practically equal at the two stations, they become absolutely eliminated in our determination of the longitude. If they be unequal, the resultant longitude will require an increase by one-half the excess of their sum for westward signals. In either case, only their total sum at the two stations is determined by the operations for longitude.

If we assume that the time lost upon the chronograph-circuit is the same at each station, the last of the above-mentioned intervals becomes eliminated by the comparison of the two records. The second and third depend upon the galvanometer and observer at the receiving station, and are not easily to be separated from each other in any determination of their amount; but if their sum can be measured, this, subtracted from our quantity x, will afford a trustworthy determination of the velocity with which the signals are actually transmitted through the telegraphic circuit.

This sum of the delays dependent on the galvanometer and the observer, I have called "the personal error of noting;" and the attempts to measure its amount have been so successful, and have manifested such an unexpected constancy in its value for different persons, at different times, and at both stations, that the results obtained for the velocity of transmission of our signals seem entitled to a high degree of confidence.

By observing a series of signals similar to those exchanged for longitude, and so arranged that both the original signal and the observation of the consequent deflection shall be recorded on the same chronograph, the desired measure may be obtained. Experiment showed at once that the interval thus determined was altogether too large for any inconvenience to arise from the use of a single recording pen. The obstacle first encountered arose from the circumstance that the minimum battery force requisite for the electro-magnet of the chronograph pen was about seventy-five times greater than the maximum which could be safely employed for

the galvanometer signals. To obviate this difficulty, a battery of two Minotti cells being employed, the circuit was divided at the galvanometer into two branches—one, of fine German silver wire, passing to the galvanometer and thence again to the main circuit, while the other branch was made to pass through the break-circuit key by means of which the deflections were noted. The resistances of these two branches were so adjusted that they were in the ratio of 1 to 100, by which device each signal at the observatory was sharply indicated on the galvanometer, without too great violence; and by a slight adjustment of the movable permanent magnets, it was always possible to render these deflections similar in amount to those received from Newfoundland. It was, of course, necessary to include the clock in the galvanic circuit, in order to obtain a time-scale; but the interruption and restoration of the circuit at each oscillation of the pendulum caused a vibration in the galvanometer needle, which was not quieted for more than half a second, and then only to be renewed immediately. To render all the circumstances of the experiment as similar to those of the longitude-signals as the nature of the case permitted, as well as to avoid any tendency to mechanical rhythm in the act of noting the signals (a source of inaccuracy which every observer by the chronographic method must have recognized whenever the beats of his clock have been audible or visible during the process of observation), it was necessary to dispense with the clock while the measures were actually in process.

The observations were therefore arranged as follows:—After the clock had been included in the circuit for some minutes, recording its beats upon the chronograph in the observatory, and manifesting them likewise upon the galvanometer in the telegraph office, the assistant in the observatory excluded the clock from the circuit by means of a plug-switch, thus stopping all record of time upon the chronograph sheet, although the pen continued to trace a straight line, and stopping likewise the pulsations of the galvanometer needle, by which indication the observer was warned that the signals were about to begin. He then gave a set of ten signals on one of the observing keys, at the same intervals, roughly, as those exchanged for longitude—namely, four sharp, quick taps upon the key, about five seconds apart; then, after ten seconds, three more; and, after another ten seconds, yet three more. At the close of this set of signals, he restored the clock to the circuit by removing the plug from the switch, and the graduation of the time-scale recommenced as before after an intermission of scarcely a minute; so that the times of each signal could be read off by means of the second-marks of the preceding and following minutes with an accuracy scarcely, if at all, inferior to that attainable when the time-record is simultaneously in progress. The chronographic records of the signals thus given are about $0^s.04$ long.

The observer is meanwhile at the galvanometer in the other building, out of sight and hearing of the assistant, and notes the moments of deflection of the light-spot by a tap upon the break-circuit key which he holds in his hand, taking care to conform in all respects to his habitudes while observing longitude-signals. The intervals between the chronographic records of the original signals and his observations of the same, then furnish a measure of the "personal error of noting" as already defined; and show the lapse of time corresponding to the sum of all the

various delays of which our x is composed, except the actual time of transmission through the cable; unless the adjustments of the two chronographic or local circuits are so diverse that the loss of time which they entail cannot be regarded as equal for the two instruments. This is not the case, since repeated examination has shown that the difference is not measurable. The exclusive employment of signals given by interrupting the galvanic circuit, and of a pen which is not removed from the paper during the whole period, renders the measurement of armature-time very easy, and eliminates it from ordinary observations.

On the 2d November I made five such determinations of my personal error of noting, each one based upon one series of signals, and with the following results. The errors appended are the mean errors of the mean, not the so-called probable errors, which would be but two-thirds as large.

	$0^s.277 \pm 0^s.013$
	$0.256 \pm .012$
	$0.230 \pm .011$
	$0.248 \pm .014$
	0.262 ± 0.018
These give the final value . .	0.253 ± 0.006

A series by Mr. Mosman gave 0.275 ± 0.014
and one by Mr. George, of the telegraphic staff, who had had no previous experience in observing, gave . . . 0.296 ± 0.017

On the 7th November, five determinations gave for my own error—

	$0^s.292 \pm 0^s.010$
	0.300 ± 0.013
	0.288 ± 0.006
	0.285 ± 0.007
	0.291 ± 0.010
the mean value being	0.289 ± 0.005

Mr. Mosman's error from four determinations being—

	0.322 ± 0.027
	0.296 ± 0.031
	0.303 ± 0.016
	0.297 ± 0.013
and Mr. George's	0.309 ± 0.022

The galvanometer was evidently somewhat less sensitively adjusted than on the previous occasion, as was indeed known independently of the signals, since it had been undergoing some repairs; yet the average excess was but three and a half hundredths of a second.

The Kessels clock, at Heart's Content, was provided with two signal-giving attachments, one being the ordinary arrangement for breaking circuit at the moment when the pendulum-rod is vertical, and an additional tilt-hammer being available for interrupting the circuit at the instant of extreme elongation on alternate seconds. Mr. Dean availed himself of this means for measuring the personal error of noting signals, by connecting each tilt-hammer with a separate circuit. One of these

passed through the normal signal-apparatus of the clock, the Morse register, the signal-key, and the galvanometer; the other through the subsidiary tilt-hammer, the observing key, and the chronograph. The original signals were thus recorded on the chronograph sheets by means of a clock-scale graduated to two seconds, while the observations of the same were registered upon the Morse fillet; and a slight change made in the connections at the close of the experiments sufficed to put the records of both tilt-hammers upon the chronograph, and thus permit an accurate measurement of the interval between the two systems of clock-signals. In the first-named circuit a battery of two carbon cells was employed, resistance-coils being interposed to reduce the deflections of the galvanometer to the magnitude of those obtained through the cable; and the chronograph magnet proved sufficiently sensitive to record these.

On November 10, five series of measures gave for his personal error—

0ˢ.22	± 0ˢ.020
0.28	± .019
0.24	± .010
0.24	± .025
0.22	± .008
or from all	0.236 ± 0.009

On the 12th November, again, his observations of twelve sets of signals give, after deducting 0ˢ.48 from each to correct for the difference of the two time-scales—

0ˢ.224 ± 0ˢ.017	0ˢ.148 ± 0ˢ.010
.244 ± .020	.195 ± .009
.193 ± .014	.208 ± .016
.181 ± .014	.271 ± .012
.170 ± .017	.239 ± .022
0.239 ± 0.016	0.209 ± 0.013

or from all . . . . . 0ˢ.192 ± 0ˢ.009

The marked inferiority of these values to those found for three observers, on two different occasions at Valencia, excited my suspicions, and on inquiry of Mr. Dean it proved that his observations had been made in the same room in which Mr. Goodfellow had given the signals, and where the click of the key was distinctly audible, so that the observation was not purely dependent upon the deflection of the needle, but was possibly influenced by the sense of hearing.

Mr. Dean therefore repeated his observations under circumstances precluding the possibility of his personal error being affected by any extraneous influence of this kind. This was done on November 17, and ten series of signals (one of the original eleven being discarded for manifest irregularity) afford the following results, in which the difference of time-scales is included:—

0ˢ.803 ± 0ˢ.027	0ˢ.832 ± 0ˢ.020
.834 .014	.795 .018
.831 .020	.857 .026
.820 .017	.870 .027
0.848 0.021	0.864 0.026

the definite value from the ten series being

0ˢ.830 ± 0ˢ.008

For the difference of the time-scales 52 comparisons, during one minute preceding the observations, give 0ˢ.491, and 60 comparisons immediately afterwards give 0ˢ.499. Adopting 0ˢ.495, therefore, as the most probable value, and deducting this from the final value 0ˢ.830, we have 0ˢ.335 as Mr. Dean's personal error in noting the signals.

The difference between this error and that found for my own observations at Valencia is small, and is probably owing to the galvanometer rather than the observer; the apparatus at Heart's Content being known to be somewhat less sensitive than that at Foilhommerum. The constancy of the error is also here strongly manifest; and the illustration of the unrecognized but marked effect of the sound of the tap, upon observations supposed to be of the visible deflection only, is instructive.

It may not be inappropriate to mention in this connection that a very marked effect upon the observation of transits of stars is likely to be produced when the chronograph is in the same apartment, so that the regular beats of the magnet are audible. When the intervals between the transit-threads are approximately multiples of half a second, the tendency is very great so to tap upon the observing key as to produce a rhythmical beat in the armature; and when the interval differs from the multiple of a second, the occurrence of that magnet-beat which records an even second often precipitates the tap of the observer, whose nerves are in keen tension awaiting the instant of bisection. Only a strong effort of will can obviate these perturbing influences—which are akin to those exhibited in the measurements just described.

The personal error of noting being then assumed as 0ˢ.271 at Valencia, and 0ˢ.335 at Newfoundland, the sum of these quantities, or 0ˢ.606, is to be deducted from our value of $x_1 + x_2$ to obtain the true time of transmission; and half their difference, or 0ˢ.032, is to be deducted from the longitude after all other corrections are applied. This correction will be taken into account, in fixing the value to be adopted.

It may be added that the indications are strong that a considerable portion of this "personal error of noting" is not strictly a personal phenomenon, but that it is due to the consumption of a very appreciable interval of time in overcoming the inertia of the needle and in moving the needle through an arc sufficient to attract attention. Indeed it is my conviction that not less that the tenth of a second is thus lost.

An automatic apparatus might be arranged, all other means failing, for recording the signals received, by adjustment of delicate silver wires on each side of the galvanometer needle, in such a position, and so connected with the battery, that they would be brought in contact whenever the deflection of the needle reached a certain angle, and the signal be thus recorded upon the chronograph. This would definitely decide the question; but, for obvious reasons, no such experiment was undertaken at Valencia. My immediate object was thoroughly attained by the satisfactory results of these measurements of the sum of all delays not due to time consumed in the actual transit of the signals across the Atlantic.

X.

PERSONAL EQUATION IN DETERMINING TIME.

In the telegraphic operations of the Coast Survey, the unvarying rule has been that the personal equation be eliminated as far as possible by an interchange of position of the two observers, and also measured at least once during the progress of each longitude-campaign, by observations specially instituted for that purpose. These determinations are made with the same instruments used for the other work, the two observers sitting side by side, and observing alternate tallies of five threads each. A pair of stars thus gives a measure of personal equation unaffected by any small error in the adopted values of the thread-intervals; since the same person who observes the first, third, and fifth tallies for the first star, observes the second and fourth tallies for the second star.

The advantages and defects of this method are evident to the astronomer at once. For the end to which it is ordinarily applied, it is especially adapted. Since the longitudes depend on transit-signals for zenithal stars, the observations for personal equation are made by the use of stars of the same class, and care is moreover taken that the magnitudes of stars employed for the two purposes shall not differ essentially from one another. On the other hand, it cannot be denied that a certain amount of nervous excitement is likely to accompany observations thus made, since the observer has usually but a short time available after bringing his eye to the telescope, before the first transit occurs.

Furthermore, the eye-piece has to be moved, to bring the new tally into the middle of the field, and the position of the body is frequently somewhat constrained in consequence of the close proximity of the two observers. The careful and long-continued study of these observations of personal differences for a considerable number of observers, during a period of about eighteen years, has thoroughly convinced me, as often stated on other occasions, that the personal equation varies decidedly with the magnitude, and very greatly with the declination of the star.

Three elements seem especially to enter into the magnitude of the personal differences in right-ascension: 1, the perceptions of the observer, which are affected by the magnitude of the star, and possibly to some extent by the rapidity of its apparent motion; 2, the habitudes of the observer, as determining the moment at which he endeavors to give his signal upon the telegraphic key; and 3, the construction and adjustment of this key itself, which affect, to a certain extent, the interval between the intention to give the signal and the complete execution of this intention. The unrecognized interval, which intervenes between the perception by sight and the performance of the consequent endeavor to press the button of the observing key, may be regarded as merged with the second of the influences above named. It forms a large portion of the theoretical personal equation, but a much smaller part of its practical amount, which is very dependent upon less subtle causes of delay.

The first of these elements of personal equation explains the difference which

certainly exists in its value for the same observer, when different instruments are employed for its measurement; the magnifying power, and the amount of light, each appearing to exert a distinct effect.

The second is a subject of considerable interest; and extended series of comparisons between the observations of the same persons, using eye-pieces of different magnifying power with the same instrument, and using instruments of different aperture with similar reticules and eye-pieces, could not fail to afford much information. It had long been my desire to carry out this investigation, toward which, indeed, a considerable amount of materials has been collected, but for the present, at least, no facilities are within my reach. It is certain that persons of the most delicate nervous organizations are not generally those who observe a transit earliest; nor does the reverse hold true. And it would seem that an influence is here involved, which does not exist in the method of observation by eye and ear; viz., an (generally unconscious) effort of judgment, by which many, if not most, observers give their signal-tap, not at the instant when the star is seen upon the thread, but at such a previous moment that the signal may in their estimation take effect at the instant which it is desired to record, after the lapse of an interval of volition and an interval of muscular contraction. It is readily seen that if an observer succeed in attaining this end for both equatorial and circumpolar stars, it can only be by a very accurate estimate of the apparent rate of motion of the star, and that a change of eye-piece for the same star will produce an effect analogous to a change of declination in the star observed. The true method to be aimed at, in chronographic observation, clearly is to give the signal at that instant when the star is actually seen to be bisected. Then, however large the personal difference from other observers, the personal equation becomes constant, unaffected by many extraneous influences, which cannot otherwise fail to exert a perturbing influence. Still, the attainment of this end is by no means entirely within the observer's control, but must, under any ordinary circumstances, vary with the organization and training of the individual. The strictly psychophysical part of the personal equation, is, as I have already remarked, merged with such other parts as depend upon the observer's habitude. Yet it is clear that all these portions are in general not constant, but vary to a great extent with the position of the star, and probably with other external circumstances. It is probably in this element, also, that the well-known variation takes place according to the condition of the observer.

The third element, the key used, is generally of more importance in those chronographs on which the signals are given by the closing, or making, of a circuit, than on our own, all of which are arranged for break-circuit signals, inasmuch as in the former case it is usually needful for the contact-piece to be moved through an appreciable space before the signal is given, while in the last-named arrangement the first motion of the contact-piece breaks the galvanic circuit, and records itself upon the chronograph. But if the spring, which maintains the contact when the button is not pressed, be stronger than usual, or not nicely adjusted, there is danger that an observer accustomed to the use of a more delicate key, upon which a touch suffices to produce an interruption of the circuit, may either fail to record his signals at all, or in default of this may consume an appreciable time in exerting suffi-

cient muscular force to produce a galvanic circuit. For the sharpest observation a delicate adjustment is requisite; yet this very delicacy of touch, which requires ordinarily no muscular effort, becomes a source of inaccuracy when the habitude of observation thus acquired is applied to a coarsely-adjusted key. From this extreme case downward, every degree of gradation may exist, and this crude source of discordance between individuals may acquire great importance, under some circumstances, when the same key is employed by different observers; since the most delicate adjustment tolerable for one person, may and often does require too strong a pressure for another's observations to be at all satisfactory.

These various considerations are here presented in some detail, inasmuch as they have proved particularly important in this investigation; in which the question of personal equation has been the most embarrassing, and in which all the considerations here presented are to be carefully weighed.

It will readily be seen that the measurements of personal difference by the ordinary method, properly and successfully used in connection with the determinations of longitude by star-signals, are inapplicable, in great measure, to determinations, like the present one, by comparison of clocks. For the clock-corrections at the two stations, upon the correctness and congruity of which the resultant longitude is dependent, are determined by the combination of transits of high and low, zenithal and equatorial stars. And the personal difference of observers for the aggregate of such observations upon stars not the same, is a quantity entirely different from that which would be deduced from, and applicable to, stars of any one particular class. Indeed, when transits of stars at declinations beyond the limit proper for chronographic determinations are combined with the time-stars in the neighborhood of the zenith or equator, the two values of the personal difference are scarcely comparable. In a word, the values applicable to the method of star-signals are inapplicable to the method of clock-comparisons, and their employment may result in not the removal, but the introduction, of error. For time-determinations in general, there are two modes in which the personal equation may be measured or eliminated. One is an interchange of stations by the observers; the other is the systematic determination of time by the two observers independently, using the same instrument and clock, and a well-determined series of stars carefully reduced to the same equinoctial points. These methods give, not the personal difference strictly speaking, but the mean value of the personal differences for such stars as are habitually employed for determining time; and either of them thoroughly applied would remove all effect of personal equation from the longitude as measured by clock comparisons. The last-named method is, as is well known, exclusively employed at Greenwich, and with excellent results.

Of course neither of these methods was available for the trans-Atlantic longitude. The remoteness of the stations from each other, and their difficulty of access precluded any undertaking of the kind, except at an inadmissible outlay of time and money. It was therefore arranged that a thorough series of comparisons between all the observers should take place at the earliest possible time after their return to the United States, and the corrections to be adopted thus determined. The misapprehension by which the intended elimination of the personal equation

of the observers at Newfoundland failed of accomplishment is attended by a minimum of embarrassment, since the equation between Messrs. Dean and Goodfellow has varied between very narrow limits, on the two sides of nothing, for a number of years.

It was found impracticable to make the arrangements for the series of personal comparisons, without fitting up a small building specially for the purpose, which could not be accomplished till the middle of April, on account of the snow and various delays. On the 9th of April the comparisons commenced, and were continued on every occasion that the extremely unfavorable weather permitted, until sixteen comparisons had been made between eight pairs of observers; four of the six observers comparing each with three others, and two of them each with two others. It was provided that a single comparison should depend upon not less than ten pairs of stars, ten transits over twenty-five threads being thus observed by each person, and that no person should take part in more than one comparison on the same night, lest the results be affected by his fatigue.

The results of these comparisons, together with their mean errors (stars between 25° and 50° being almost exclusively used), are as follows:—

Gould—Dean	=	+0ˢ.427 ± 0ˢ.034	April 13
		+ 0.380 ± 0·026	18
Gould—Mosman	=	+ 0.472 ± 0.028	May 23
		+ 0.459 ± 0.070	28
Gould—Chandler	=	+ 0.190 0.037	June 1
		+ 0.202 0.033	19
Dean—Goodfellow	=	— 0.013 0.023	April 9
		— 0.008 0.024	11
Dean—Mosman	=	+ 0.109 ± 0.014	19
		+ 0.094 0.024	23
Boutelle—Goodfellow	=	— 0.134 0.029	19
		— 0.146 0.029	23
Boutelle—Chandler	=	— 0.147 0.028	11
Goodfellow—Chandler	=	— 0.021 0.032	13
		— 0.072 ± 0.026	April 18

Farther comparisons between Messrs. Boutelle, Mosman and Chandler, were contemplated, but were prevented by duties which called two of these gentlemen away, before farther observations could be obtained. One comparison between Mr. Chandler and myself was rejected for manifest error, on a night when the stars were only visible between rapidly flying clouds.

Assigning to these several determinations their appropriate weights and equating, we arrive at the following values—

Gould—Dean	=	+ 0ˢ.303
Gould—Mosman	=	+ 0.454
Gould—Chandler	=	+ 0.216
Dean—Goodfellow	=	— 0.029
Dean—Mosman	=	+ 0.121

Boutelle—Goodfellow	=	— 0ˢ.132
Boutelle—Chandler	=	— 0.223
Goodfellow—Chandler	=	— 0.090

or reducing to Mr. Goodfellow, as the standard of comparison,

Goodfellow—Gould	=	— 0ˢ.304
Goodfellow—Mosman	=	+ 0.150
Goodfellow—Dean	=	+ 0.029
Goodfellow—Chandler	=	— 0.090
Goodfellow—Boutelle	=	+ 0.132

In considering these quantities, the attention is at once attracted by the unusual magnitude of some of them, by the excessive tardiness of my own signals as compared with those of the other five observers, and by the fact that the personal differences in ordinary time-determinations had not been comparable with those here deduced. For example, although my own observations have usually been somewhat later than those of the many others with whom I have measured personal equations on past occasions, there is no room for the hypothesis that my difference from Mr. Mosman can have reached the enormous value of nearly half a second for chronographic observations. Indeed, a very thorough study of our observations at Valencia established the fact, that it must certainly have been less than 0ˢ.05 upon those occasions when observations were made by both of us during the same night.

A similar inference is deducible from a comparison of the longitude-results themselves. Thus, the time being determined by myself alone for the first series of exchanges, the resultant value for the longitude between Foilhommerum and Heart's Content is 56ˢ.477; for the second series, where the clock-correction is derived from interpolation between one determination by myself alone, and one made by Mr. Mosman and myself jointly, the deduced value is 56.487; while the mean of the other three series, all which depend upon time determined by Mr. Mosman alone, gives 56.465, and one of these three gives 56.481. Since the observer at Newfoundland was the same for all five series, it is very evident that no decided personal difference existed between Mr. Mosman and myself. That it could have amounted to one-tenth part of the value deduced on the 23d and 28th of May at Cambridge, is totally out of the question.

So too with Mr. Chandler's comparisons, which indicate for him a habit of observing nearly a quarter of a second later than Mr. Mosman, although more than two-tenths of a second earlier than myself. Until he went to Calais, he had observed exclusively with the same signal-key which I have employed at Cambridge; and at Calais his key was similarly adjusted. And during a very considerable series of observations with a large transit-instrument during the last two years, in which Mr. Chandler took part, I had convinced myself that so large a difference as one tenth of a second between our observations was out of the question. Yet in the present comparisons my observations were recorded later than his, by more than two-tenths of a second.

The difference between Messrs. Chandler and Boutelle seems, from examination of the Calais record, likewise to have been by no means so large as these special observations would indicate. A series of similar observations with the large transit

instrument of the Coast Survey on four nights immediately after the close of the comparisons just described—using delicately adjusted keys, to which both of us were accustomed—gave as the difference between Mr. Chandler and myself

$$\text{Gould—Chandler} = -0^{s}.021,$$

instead of + 0.216 as above; while the difference between Mr. Boutelle and myself, as measured in past years, has rarely attained the limit of $0^{s}.2$.

The comparisons between Messrs. Dean and Mosman seem to have been similarly, although not equally affected by the same cause; and I have thus been led to the conviction that but little, if any, weight ought to be assigned to these determinations of personal equation, as regards their application to the clock-errors, from which the longitude must be deduced. If farther argument were needed, it would only be necessary to apply to the series of preliminary results already deduced in Chapters VII. and VIII., the values of personal difference here obtained. The accordance, now so satisfactory, would be entirely destroyed; and the probable error of the result increased more than tenfold, for each of the two longitudes.

The difference here found between Messrs. Dean and Goodfellow is the only satisfactory one. These gentlemen have been accustomed to observe in connection with one another for ten or twelve years; and a very extensive series of measurements, both by observations specially made for the purpose, and by the comparison of longitude-results deduced from their observations before and after exchanging stations, shows that their personal difference has usually scarcely exceeded the limits of probable error, while it has varied in sign, as already stated.

A satisfactory explanation of the phenomenon is, I think, to be found in the break-circuit keys employed, of which the springs were so strong as to prompt a memorandum on each date when I observed, to the effect that my observations were embarrassed by the strong tension of the keys, which were those used at Newfoundland. Many of my observations were lost in this way at the commencement of the work, and my first night's comparisons proved futile for this reason; inasmuch as the greater proportion of my signal-taps were found not to have been recorded at all upon the chronograph, which was in another building, some twenty-five rods distant. My pressure upon the button had not been forcible enough to break the contact. Mr. Boutelle also complained of the stiffness of the observing key, and caused a note to this effect to be entered upon the journal of the observations for personal equation.

Under these embarrassing circumstances only two courses seem to be available. A repetition of the comparisons, using more delicate signal-keys, would have been highly desirable, and was earnestly hoped for; but, apart from the other serious obstacles, the assignment of the various observers to other duties, some of them at very remote stations, precluded all possibility of this solution of the difficulty. We may however totally discard all consideration of the personal equation, except the value between Dean and Goodfellow, which latter may be regarded as so small and well established as to reduce nearly to a minimum the effects of the misapprehension by which the time-determinations, at Calais, for the two steps in the longitude, were made by different persons; or, on the other hand, we may fix upon approximate values, by considering the tolerably accordant determinations made at

other times, and comparing likewise the transit-observations made by different persons at the same station, during the present longitude-operations.

The latter course seems preferable, and all the more allowable, inasmuch as those values which careful, independent scrutiny has rendered the most probable are all of them small, yet most of them distinctly indicated. And I propose to adopt, as not altogether empirical, although obtained by an exercise of judgment quite as much as of computation, values for the personal equations, deduced from other sources than the special comparisons here described. It so happened that the algebraic signs of the numerical values thus employed are the same as by the special comparisons, although the magnitudes of these values are very much diminished.

I cannot but believe that an explanation is here presented of the very perplexing phenomenon, so often, and indeed so generally, encountered in the discussion of personal equations, that the values, as found from the comparison of two observers directly, differ so widely from the results obtained when a third observer is employed as an intermediate standard. Different individuals are affected, by any unusual circumstances attending their observations, in degrees differing with their nervous organizations.

Thus, in the present case, Mr. Mosman's observations were probably affected but slightly by the stiffness of the key-spring, which apparently affected those of Messrs. Boutelle and Chandler and myself to so great an extent.

The following values have been adopted, as seeming most truly to represent the personal equations between the different observers, while engaged in the regular observations of the campaign:—

Gould—Mosman	=	+ 0ˢ.02
Dean—Mosman	=	+ 0.11
Goodfellow—Dean	=	+ 0.02
Boutelle—Goodfellow	=	— 0.14
Boutelle—Chandler	=	— 0.04

While adopting these values, I am far from believing that they are the same for stars in different declinations, or even for stars of different magnitudes. But they do seem to represent, with some approximation to the truth, the average differences between the several observers in determining time.

XI.

FINAL RESULTS FOR LONGITUDE.

1. *Foilhommerum and Heart's Content.*

The longitude deduced from the signals of Oct. 25 depends upon time-observations at Valencia by myself, and may therefore be combined with those of the last three nights on which Mr. Mosman determined the time, by subtracting the adopted personal equation, Gould—Mosman = + 0ˢ.020. But the longitude of Oct. 23 depends upon the transit-observations of Oct. 28 and 30, on the latter of which dates three of the nine stars were determined by Mr. Mosman. Applying to the

observed times of these three stars the correction + 0ˢ.020, and repeating the solution for two unknown quantities, we shall find the azimuth correction A to be changed by + 0ˢ.011, and the clock-correction Δt by — 0ˢ.009. This increases the interpolated values for the Valencia clock-corrections during the period of the telegraphic exchanges by only 0ˢ.001, making the resultant longitude larger by this amount, and the subtraction of 0ˢ.020 from the result refers the whole series to the observations of Mosman at Valencia, and Dean at Newfoundland, as follows:—

1866. Oct.	25	$2^h\ 51^m\ 56^s.457$
	28	.468
Nov.	5	.455
	6	.481
	9	.460

The sum of the squares of the deviations of the several values from their mean is thus slightly reduced. An equal weight seems fairly attributable to all the determinations, excepting the first, in which there is a regular increase in the values deduced from the successive sets, which possibly indicates a variability in the clock-rate. This, together with the want of experience necessarily attendant upon the first trial, leads me to assign to it but half the weight given to the other four, and we thus attain the mean value of the longitude.

$$\lambda = 2^h\ 51^m\ 56.465$$

which, corrected for the personal equation in determining time Dean—Mosman = + 0ˢ.11, and for that of noting signals Dean—Gould = + 0ˢ.03, becomes

$$\lambda = 2^h\ 51^m\ 56.54.$$

2. *Heart's Content and Calais.*

The time-observations from which the longitude between Heart's Content and Calais is deduced were made by Mr. Boutelle for the second and third series of exchanges, and by Mr. Chandler for the first and fourth. The resultant values on the 11th and 16th December require, therefore, the subtraction of the correction, Boutelle—Chandler = — 0ˢ.04; after which the several determinations may be combined, to obtain the value which would have been found, had all the Calais observations been made by Mr. Boutelle alone.

The result of the exchanges, Dec. 12, is very far from trustworthy, as a glance at the computation of the time will show. During the three hours which were requisite for obtaining the transits of seven stars at Calais, the clock lost 1ˢ.28, although it had gained 0ˢ.4 during the eleven hours preceding, and gained again during the two hours following. Some serious disturbance to the clock evidently occurred about this time. The unfavorable weather prevented Mr. Boutelle from detecting it, in spite of his best endeavors; but the fact is not surprising in a clock so old, and so ill adapted for transportation. It would seem as though the fault were in the compensation; but examination has shown the teeth of the seconds-wheel to have been in bad order, so that a "jump may have occurred during the course of the observations, without detection at the time, or recognition in the

transit-observations themselves." At any rate the result obtained from the exchanges of Dec. 12 seemed entitled to small reliance, before its large discordance from the other values was manifest.

Reducing all the values to Mr. Boutelle, and rejecting that of December 12 from the mean, we thus obtain:—

December 11,	$0^h\ 55^m\ 37^s.93$
12,	[37.53]
14,	37.84
16,	37.82
Mean,	0 55 37.86

which diminished by $0^s.14$ to correct for the personal equation between Messrs. Boutelle and Goodfellow, becomes—

$$\lambda = 0^h\ 55^m\ 37^s.72.$$

3. *Greenwich and Foilhommerum.*

It has been already stated that the Astronomer Royal cordially acceded to my request that he would take measures for the determination of the longitude between Greenwich and our station at Foilhommerum. This request was made with diffidence, since Mr. Airy had already determined the longitude of two other points in Valencia with all possible care,—Feagh Main, the highest point on the island, having been measured chronometrically in 1844, and Knightstown telegraphically in 1862,—so that the establishment of our station at Foilhommerum implied the determination of an additional arc in order to connect it with Greenwich, whereas we had hoped to adopt the old station of the Astronomer Royal at Knightstown, six miles to the eastward.

The arrangements for the telegraphic interchange of signals with Greenwich were made by Mr. Airy, and the reductions were executed under his direction at the Royal Observatory; our own share in the work being limited to the operations at Foilhommerum. Exchanges were attempted on ten nights between the 3d and 15th November, but were successful only on the 5th, 13th, and 14th. On the last occasion the weather precluded us from obtaining any observations for time, so that the result depends upon two nights' exchanges. These proved, however, very accordant.

The clock at each terminus was made to record itself upon the chronograph at the other for half an hour, and the construction of the chronographic and signal-giving apparatus at Greenwich required our clock-signals to be given by closing an open circuit, not by interrupting a closed one, and the Greenwich signals to be received in a similar way. To meet this need, the relay-magnet was modified, while receiving signals, by transferring the conducting-stop of the armature to the rear, so that the currents arriving at each second should interrupt the local circuit of the chronograph-magnet like our own clock-signals. And in sending our signals to Greenwich the connections of the main and local circuits with the relay-magnet thus modified were respectively reversed, so that an interruption of the local circuit by our own clock produced a closure of the main circuit, which transmitted a current to Greenwich. Thus no loss of time was entailed in receiving signals;

but, in sending them, an armature-time intervened between the actual clock-signal and its transmission to Greenwich. This was reduced to a minimum by strong tension of the spring, and two series of experiments were made to measure the amount of the delay.

For this purpose, the relay-magnet being retained in the chronograph-circuit in the same manner as during the transmission of signals to Greenwich, the two terminals of the instrument (which are in permanent connection with the armature and its conducting stop, and which, during the sending of signals, are connected with the two wires of the main line) were also brought into communication with the chronograph-circuit on the two sides of the recording magnet. The effect of this arrangement was, that when the clock-signal, which is of course recorded upon the chronograph, released the armature of the relay-magnet by interrupting the galvanic circuit, this armature on its arrival at the outer stop completed a metallic connection by which the chronograph was excluded from the circuit. This was recorded upon the chronograph, like a second interruption, which continued until the tension of the spring was overcome by the re-established current. In this manner two signals were given in each second; the first by the clock directly, the second by the relay after the lapse of the interval required for the armature to reach the outer stop. Then, if the chronograph-magnet be adjusted with all possible delicacy, the length of the record of the total interruption must be increased by the full amount of the delay in question. Series of observations were made for the investigation of this point on the 4th and 14th of November, and indicate a delay of $0^s.02$ in the communication of signals, being equivalent to a retardation of the clock by this amount in the currents sent, though not in their record; and implying a diminution both of the longitude and of the transmission time by $0^s.01$.

The longitude as deduced from the two nights' exchanges is:—

	λ	x	Number of signals. Greenwich.	Valencia.
1866, November 5,	$0^h\ 41^m\ 33^s.305$	$0^s.115$	66	210
13,	33.280	0.110	80	70
the mean being,	0 41 33.29			

The Greenwich observations were made by different persons on different nights, but were all reduced to Mr. Dunkin in the usual manner.

The line of telegraph passed through Killarney and Mallow to Dublin, thence to Wexford, St. David's, Cardiff, London, and Greenwich. Its total length must have been very nearly 600 miles (966 kilometers), exclusive of the submarine cable between Ireland and Wales, which is about one-tenth part as long. The length of the cable across the straits of Valencia is about three-quarters of a mile.

Referring the longitude of Valencia to Feagh Main, as the fundamental point adopted for the great European Arc of Parallel, by means of geodetic reduction of the telegraphic stations, Mr. Airy finds for the longitude of this point west of Greenwich—

1. By the great chronometric expedition of 1844, the transit instrument being placed in the station of the trigonometrical survey			$0^h\ 41^m\ 23^s.23$
2. By the telegraphic communication of 1862, the time instrument being placed at Knightstown,			
Greenwich to Knightstown . .	$0^h\ 41^m\ 9^s.81$		
Reduction to Feagh Main . .	+ 13.56		
			0 41 23.37
3. By this telegraphic communication in 1866, the transit-instrument being placed at Foilhommerum,			
Greenwich to Foilhommerum . .	$0^h\ 41^m\ 33^s.29$		
Reduction to Feagh Main . .	— 10.10		
			0 41 23.19
From which he adopts			
Feagh Main west of Greenwich			0 41 23.29

The variation of these measures may be accounted for in great degree by the local deviations of the direction of gravity in this hilly region, and their consequent effect upon the geodetic reductions.

4. *Final Inferences.*

The combination of the three longitudes thus determined, gives—

Greenwich—Foilhommerum,	0^h	41^m	$33^s.29$
Foilhommerum—Heart's Content,	2	51	56.54
Heart's Content—Calais,	0	55	37.72
Greenwich—Calais,	4	29	7.55

The Valencia observations having been made by, or referred to, Mr. Mosman throughout the whole period, his personal equation is eliminated; the equation between Messrs. Goodfellow and Dean, always small, may be regarded as trustworthy, and by a happy coincidence the personal equations of Mr. Boutelle on the west, and of Mr. Mosman on the east, seem to be almost identical, so that even a total disregard of this quantity would have resulted very nearly in its perfect elimination, the oceanic arc being diminished and the land arc increased, each by about $0^s.14$.

The only probable influence of personal equation in the entire longitude-measurement, comprising, as it does, three-sixteenths of the whole circumference, lies in the difference between the observations of Messrs. Dunkin and Boutelle.

The longitude of Calais, as heretofore telegraphically determined, is as follows:—

Calais—Bangor,	0^h	6^m	$0^s.31$
Bangor—Cambridge,	0	9	22.99
Cambridge—New York,	0	11	26.07
New York—Washington,	0	12	15.47
Calais—Washington,	0	39	4.84

whence we have

Greenwich—Washington,	5^h	8^m	$12^s.39$

The Seaton Station being $12^s.44$, and the dome of the Capitol $10^s.17$, east of the

Naval Observatory, to the centre of the dome of which the preceding value refers, we have as their longitudes from Greenwich—

Seaton Station,	$5^h\ 7^m 59^s.97$
Capitol,	5 8 2.22

XII.

TRANSMISSION-TIME OF THE SIGNALS.

We have seen in Chapter VII how an interchange of signals gives the numerical measure of the time consumed in their transmission and registration, upon comparison of the records at the two stations. Representing the clock-time and its needful correction by T and Δt, denoting the signals from Valencia and from Newfoundland by the subjacent figures 1 and 2 respectively, and distinguishing by an accent those quantities which depend upon the Newfoundland clock, we have (since the Valencia signals preceded)—

$$x_1+x_2=(T_2-T_1)-(T_2'-T_1')+(\Delta t_2-\Delta t_1)-(\Delta t_2'-\Delta t_1')$$

or, in words: the sum of the transmission-times for westward and eastward signals, each increased by the error incurred in the process of recording, is equal to the excess of the recorded interval upon the chronograph at the station whence the first signal was given, increased by the excess in the loss of time by the clock at that station during the interval.

In our experiments the interval in question rarely amounted to so much as 160 seconds, and the clock-rates were small. The correction due to difference of rates appears never to have surpassed the thousandth of a second; and, since it is certainly a quantity of the second order in comparison with the variation in personal error, we may disregard it, and consider the quantity x_1+x_2 as the excess, in the record upon the eastern chronograph, of the interval between the westward and eastward signals. Or, otherwise stated, it is the excess, for eastward signals above westward ones, of the difference of time recorded upon the two chronographs.

Half of this excess would measure the time required for the transmission and record of a signal, assuming the velocity to be the same in each direction, could we assume the personal error in noting to be equal for the two observers. This we have in Chapter IX found not to be the case, but happily we have trustworthy values of the absolute amount of the error for each observer. Deducting the sum of the two errors from the quantity x_1+x_2, we have determinations of the actual time consumed in one westward and one eastward transmission; or, if we assume the velocity in each direction to be the same, we have the measure of twice the time required for the transmission of a signal through the length of the telegraphic cable.

The transmission-time as determined for the dates of the several longitude-determinations has been deduced in Chapter VII, subject to a correction for the mean personal error in noting signals, which correction we have in Chapter IX found to be $0^s.303$. Applying this to the results obtained, we have the following values for the mean time of transmission of signals, upon the five nights when the longitude was determined:—

1866, October	25,	0".314	Cable of 1865, with earth and condenser.
	28,	.343	" " " " " "
November	5,	.280	Both cables, no earth.
	6,	.248	" " " "
	9,	0.240	" " " "

The battery-strength on these nights was as follows:—

October	25,	10 cells at Valencia,	10 cells at Newfoundland.
	28,	10 " " "	10 " " "
November	5,	3 " " "	3 " " "
	6,	3 " " "	10 " " "
	9,	4 " " "	10 " " "

It was my intention that the battery employed at Newfoundland should in every case be of equal strength with that used at Valencia; but, through misapprehension on the part of the observer at Heart's Content, this was not the case on either of the last two of the five nights of our longitude-exchanges. Yet from the results just given, the inferences seem warrantable, 1st, that the velocity of transmission is greater when the circuit is direct and consists of a good metallic conductor exclusively, than when the signals are given by induction, although the earth may be at the other electrode; and 2d, that an increase of intensity in the electromotive force is attended by an increase in the velocity of propagation of the signal.

From the beginning it was part of my design to arrange and make a system of experiments for obtaining general answers, so far as might be possible, to sundry interesting questions to which previous investigations had afforded no satisfactory replies. Among these were—

1. The character of the agency which gives the telegraphic signal upon the closing or interruption of the galvanic circuit, and the route by which its transmission is effected.

2. The influence exerted upon the conductor by using the earth as part of the circuit, or by placing the complete circuit in electrical communication with the earth.

3. The extent to which the velocity of propagation of the signals is dependent upon the intensity of the electromotive force and upon the resistance of the conductor.

4. The equality or difference in speed of the signals from the positive and from the negative electrode, when the other is connected with the earth; as also the relative velocity of signals given by completing and by interrupting the circuit.

Of course it was not to be expected that satisfactory information could be obtained, or crucial experiments devised regarding all these points; but these were the guiding ideas in providing for the additional experiments, which were carried out with the friendly aid of the gentlemen of the telegraphic staff on the 1st, 10th, and 16th of November.

The length of the cable of 1865 is 1896.5 nautical or 2186 statute miles, and that of the cable of 1866 is 1851.6 nautical or 2134 statute miles. Expressed in metric units, the cable of 1865 is 3518 kilometers, and that of 1866 is 3435 kilometers long.

In each cable the conductor is formed by six copper wires twisted around a seventh one. It has a diameter of 0.147 inch or 3.7 millimeters; and weighs 300 pounds to the nautical mile, or 73.476 grams to the meter. The copper was guaranteed by the manufacturers to have a chemical purity of 85 per cent., and its specific conducting power (that of pure copper being 100) was found by test to be 93.1 for the cable of 1865, and 94.6 for that of 1866. Its specific gravity as determined by Mr. Willoughby Smith was 8.90.

The electrical tests of the cables, after they were laid and in complete working order, had been made by Mr. Latimer Clark, a short time previous. They gave the following values, expressed in terms of the standard units,[1] adopted by the British Association for the Advancement of Science, and which promise to become generally accepted, as a peculiarly convenient system of electrical measurement.

The cable of 1865 gave[2] a resistance of 4.01 ohms to the knot; the "insulation," or resistance of the coating, being 2945 megohms to the knot, and the electrostatic capacity 0.3535 farad to the knot, or about one farad to each $3\frac{1}{4}$ statute miles.

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[1] This excellent system of measures is derived from the absolute electrodynamic units of Weber, by multiplying them by such powers of 10 as shall refer them to a convenient scale.

The unit of force f is that force which, acting during 1 mean second upon a mass weighing 1 gram, would impress upon it a velocity of 1 meter in 1 second. It differs from the meter-gram, which is the force requisite for lifting a gram through a meter in a second, and is 9.80868 f.

The unit of current c is that current which acting through 1 meter, at 1 meter distance exerts the force f upon a similar current. It decomposes about 92 milligrams of water in each cell in a second, consuming about one-third of a gram of zinc.

The unit of resistance r is the resistance of the conductor which transmits the current c in 1 second.

The unit of electromotive force e is the tension which maintains the current c with the resistance r.

The unit of quantity q is that amount of electricity which flows in the current c during 1 second.

These measures, 'absolute' in so far as they depend only upon the gram, the meter, and the second, are referred to convenient scales in the British Association's system; the measures adopted being named in honor of eminent discoverers in electrical science, in accordance with a suggestion of Mr. Clark.

The measure of electromotive force is $10^8 f$, or one hundred thousand times the absolute unit.

This has about 0.927 the tension of a Daniell's cell, and is called a *volt.*

The measure of resistance is $10^7 r$, or ten million times the absolute unit.

This is about 1.0456 times the unit adopted by Siemens, and is called an *ohm.*

The measure of quantity is $10^{-8} q$, or the hundred millionth part of the absolute unit.

This is called a *farad.*

Consequently, with a tension of one volt, and a resistance of one million ohms, the quantity of electricity would be one farad in each second.

Moreover, since the volt-farad is $10^{-8} f. q$, we have 1000 volt-farads = the absolute unit of work; or 9808.08 volt-farads per second = the meter-gram.

One million of ohms is conveniently designated as a *megohm;* and one million of farads as a *megafarad.*

[2] In the manufactory, the resistances found in each knot, at the temperature 75° Fahr. were 4.27 and 4.20 ohms, for the two cables respectively; and the respective insulating capacity of the coverings, 349 and 342 millions of ohms to the knot. These data show an increase of conducting power by 6 per cent. for the cable of 1855, and 8 per cent. for that of 1866; while the insulation had been increased in the ratios of 8.44 and 7.13. Hence, we may roughly infer the average temperature of the cables to be not far from 5° Centigrade in their ocean bed.

The cable of 1866 showed for each knot a resistance of 3.89 ohms, and an insulation of 2437 megohms; the electrostatic capacity being essentially the same as in the other.

Thus we have in the cable of 1865, as the total resistance to conduction, about 7650 ohms; as the total resistance of the insulator 1 505 000 ohms; as the total electrostatic capacity about 670.4 farads. In the cable of 1866 the total resistance is about 7270 ohms; the total insulation 1 316 000 ohms; the total electrostatic capacity 654.5 farads.

The battery employed by the telegraph company was composed of what are known as Minotti's cells; these being a modified form of Daniell's, in which the zinc rests upon a column of wet saw-dust at the bottom of which is a layer of sulphate of copper, and a copper disk being at the base of all. My friend Mr. M. G. Farmer, to whom I applied for information, found by experiment the electromotive force of one of these cells to vary from 0.75 to 0.95 volt, averaging 0.84; while the average of four Daniell's cells of ordinary construction gave 0.923 volt. Hence he estimates that, after the full strength of the current is developed, one cell should give, upon one cable with earth-connection, about 110 farads in a second.

The experiments made for measuring the velocity of signals it will be well first to describe in their regular order.

On the night of November 1, the first essays were made, after the use of an electro magnet had proved hopeless; but owing to numerous difficulties incident to a first trial, only a few signals were exchanged. These were made by employing a battery of 20 cells at Valencia, having its positive electrode to the cable of 1866, while the two cables were connected at Newfoundland without battery, and the signals thence were given by alternately breaking and closing the circuit. In the first set no communication was made with earth; 18 signals from Valencia, and 7 from Newfoundland being recorded at both stations. In the second the zincode of the battery was connected with the ground as well as with the cable; and of the signals thus given, 13 from Valencia and 3 only from Newfoundland were thus recorded.

On November 10, the first two series of experiments were successfully made, as previously arranged in the programme, excepting that during the second series the Newfoundland battery remained without change, Valencia using 4 cells, and Newfoundland 20. On November 16 the last two series were carried out, with 4 cells at each station.

On the 16th, an independent series of experiments was also instituted by causing the cables to be connected without battery at Newfoundland, while signals were given and observed at Valencia, with resistances of various amounts introduced in the circuit, and with variations in the battery power.

The first question to be investigated is, whether the positive and negative signals were transmitted with the same velocity. For deciding this, no knowledge of the actual time of transmission is requisite, but a simple comparison of the records of the same signals at the two stations will afford an answer. This comparison gives us the interval $T—T''$ (the difference of the time indicated at the same moment by the two clocks) diminished by the time of transmission in the case of signals given

from Valencia, and increased by this amount for signals from Newfoundland. This interval is a measure of the longitude, uncorrected for clock-errors or for transmission-time; but for our present purpose its absolute magnitude is unimportant, since our inquiry is answered by comparing the results deduced from positive and from negative signals with each other. Any excess of the time consumed in the passage of either class of signals should manifest itself by a superior value in the measures of the temporary clock-difference derived from that class, when the signals are sent westwardly. For eastward signals the reverse holds.

It had been intended, as will be seen from the original programme, to measure the velocity of signals while the batteries at both ends were included in the circuit, as well as when only one was employed; but since the construction of the signal-keys rendered this arrangement difficult, and inconvenient in many respects, the plan was not carried out. In all cases the battery at the receiving station was cut off from the circuit. Consequently all our experiments may, so far as regards the point now in question, be arranged in four classes, according to the character of the ground-connection. When, as in the last three of these classes, both cables were included in the circuit, those signals are called positive which put the copper of the Valencia battery to the cable of 1865, or the copper of the Newfoundland battery to the cable of 1866.

A. CABLE OF 1865, ONLY, USING CONDENSERS.

	From Positive Signals.		From Negative Signals.		No. cells.	Excess for positive.
	No.	Mean interval.	No.	Mean interval.		
Valencia signals.						
October 25, Longit.	30	$2^h\ 52^m\ 9^s.041$	28	$2^h\ 52^m\ 9^s.041$	10	$0^s.000$
28, "	29	10.107	30	10.120	10	—0.013
Newfoundland signals.						
October 25, Longit.	30	10.277	29	10.293	10	—0.016
28, "	28	11.110	27	11.096	10	+0.014

B. BOTH CABLES; MIDDLE OF BATTERY TO GROUND.

	From Positive Signals.		From Negative Signals.		No. cells.	Excess for positive.
	No.	Mean interval.	No.	Mean interval.		
Valencia signals.						
November 10, I. 1,	8	$2^h\ 52^m\ 20^s.862$	9	$2^h\ 52^m\ 20^s.844$	4	$+0^s.018$
10, II. 1,	8	20.887	9	20.864	4	+0.023
16, IV. 1,	8	21.179	10	21.163	4	+0.016
Newfoundland signals.						
16, III. 1,	8	22.234	8	22.262	4	+0.028

C. BOTH CABLES; ZINCODE TO GROUND.

	From Positive Signals.		From Negative Signals.		No. cells.	Excess for positive.
	No.	Mean interval.	No.	Mean interval.		
Valencia signals.						
November 1, I. 2,	9	$2^h\ 52^m\ 14^s.119$	9	$2^h\ 52^m\ 14^s.124$	20	$-0^s.005$
10, I. 2,	9	20.692	10	20.869	4	—0.177
10, II. 2,	7	20.693	9	20.850	4	—0.157
Newfoundland signals.						
16, III. 2,	9	22.290	10	22.315	4	+0.025

D. No GROUND-CONNECTION WHATEVER.

		From Positive Signals. No.	Mean interval.	From Negative Signals. No.	Mean interval.	No. cells.	Excess for positive.
Valencia signals.							
November 1,	I. 3,	9	$2^h\ 52^m\ 14^s.112$	9	$2^h\ 52^m\ 14^s.122$	20	$-0^s.010$
5,	Longit.	29	17.294	29	17.294	3	0.000
6,	"	18	17.203	16	17.214	3	—0.011
9,	"	30	20.292	28	20.290	4	+0.002
10,	I. 3,	10	20.748	10	20.790	4	—0.042
10,	II. 3,	9	20.752	10	20.821	4	—0.069
16,	IV. 3,	9	21.157	10	21.161	4	—0.004
Newfoundland signals.							
November 5,	Longit.	30	18.465	28	18.482	3	—0.017
6,	"	20	18.302	20	18.312	10	—0.010
9,	"	30	21.369	30	21.365	10	+0.004
10,	II. 1,	10	21.902	10	21.915	20	+0.013
10,	II. 2,	10	21.926	10	21.939	20	+0.014
10,	II. 3,	10	21.922	10	21.918	20	—0.004
16,	III. 3,	10	22.285	10	21.277	4	—0.008
16,	IV. 3,	9	22.287	10	21.266	4	—0.021

Our mean values have here been recorded to thousandths of a second—a degree of precision which is of course only nominal, since the accuracy attainable by the mode of observation employed would scarcely warrant any reliance even upon the second decimal for the mean of a number of observations much larger than ten. Yet, if this be borne in mind, no error can result from the employment of three decimals; while, on the other hand, this affords a reciprocal control in the figures.

It is manifest that if we disregard the signals given from Valencia while the zincode was connected with the ground on the 10th November, all the differences are of an order of magnitude which justifies the assumption, already probable from theoretical considerations, that the positive and negative signals travel with equal velocity under the same circumstances. This assumption I will therefore make, postponing any remarks concerning the discordance manifested on the 10th November.

The speed of the two kinds of signals being thus taken as the same under similar circumstances, the time required for their transmission is easily deduced, being one-half the difference between the measures of longitude as derived from the records at the respective stations. The weak point in our determination is, of course, the absence of any automatic record of signals received; but the considerations already presented in the chapter on Personal Error in Noting Signals afford ground for confidence that the uncertainty here introduced is comparatively small, and that the aggregate personal error of the two observers is very close to $0^s.606$. This value is adopted in the present investigation, and all the measurements hereinafter recorded, with which this personal error is merged, have been corrected by deducting this quantity.

Then for a circuit formed by both cables, without earth-connection, we have the following determinations of the sum of the transmission-times for eastward and westward signals, derived from the last three series of longitude-determinations, and from the second and fourth series of special experiments.

B. MIDDLE OF BATTERY TO GROUND.

1866.	Positive signals. No.	$x_2 + x_2$	Negative signals. No.	$x_1 + x_2$	Mean. $x_1 + x_2$	No. of cells. Val.	Newf.
II. 1. Nov. 10.	5	0s.396	5	0s.432	0s.414	4	20
	5	0.422	5	0.458	0.440		
	10	0.409	10	0.445	0.427		
IV. 1. Nov. 16.	...	.....	5	0.524	.....	4	4
	...	.....	4	0.558	.....		
			9	0.541			

C. ZINC TO GROUND.

II. 2. Nov. 10.	4	0.553	4	0.502	0.528	4	20
	3	0.719	5	0.468	0.562		
	7	0.624	9	0.483	0.545		
IV. 2. Nov. 16.	...	.....	5	0.550	.....	4	4
	...	.....	5	0.486	....		
			10	0.518			

D. NO GROUND CONNECTION.

Longit. Nov. 5.	10	0.562	10	0.617	0.590	3	3
	10	.532	10	.578	.555		
	9	.570	9	.612	.591		
	29	0.555	29	0.602	0.579		
Longit. Nov. 6.	10	0.513	9	0.518	0.515	3	10
	8	.494	7	.458	.476		
	18	0.504	16	0.488	0.496		
Longit. Nov. 9.	10	0.464	10	0.446	0.455	4	10
	10	.472	10	.508	.490		
	10	.500	10	.489	.494		
	30	0.479	30	0.481	0.480		
II. 3. Nov. 10.	5	0.572	5	0.482	0.532	4	20
	3	.577	4	.506	.536		
	8	0.574	9	0.494	0.534		
IV. 3. Nov. 16.	4	0.554	5	0.540	0.547	4	4
	5	.494	5	.458	.476		
	9	0.524	10	0.499	0.511		

And for a single cable (that of 1865) which went to earth at one end, while at the other the electrical equilibrium was disturbed only by means of a condenser through which the battery acted inductively, so that no real charge entered or left the cable at the signal station, we have from ten cells at each station—

A. INDUCED CURRENT.

	Positive signals.		Negative signals.		Mean.
1866.	No.	$x_1 + x_2$	No.	$x_1 + x_2$	$x_1 + x_2$
Oct. 25.	10	0ˢ.648	8	0ˢ.659	0ˢ.653
	10	.617	10	.675	.646
	10	.594	10	.577	.584
	30	0.620	28	0.635	0.627
Oct. 28.	9	0.794	9	0.707	0.750
	9	.691	10	.667	.679
	10	.637	9	.627	.632
	28	0.705	28	0.667	0.686

Let us now consider the experiments made without any earth-connection whatever, and first those of November 5 and 16, on which occasions the battery-power at the two stations was the same. Each station sent signals with a battery of 3 Minotti's cells on the 5th, and 4 on the 16th, receiving them with its battery disconnected. The circumstances at the two stations were as nearly identical as possible, and the mean interval consumed in the transmission of the signals appears to have been 0ˢ.29 on the former, and 0ˢ.26 on the latter occasion.

With a battery of 3 Minotti's cells, each possessing a tension of 0.84 of a volt, and incapable of generating more than 110 farads to the second when circuit was made through earth and one cable only, the maximum permanent current would not exceed 168 farads in the joined cables, and to develop nine-tenths of this current more than 1¼ second would be needed. With 3 Daniell's cells the maximum current would not exceed 185 farads. Assuredly we cannot suppose that in the lapse of three-tenths of a second, when not more than one-seventh of this current had been developed at the farther station, this battery could have charged the two joined cables, each of which possessed an electrostatic capacity of more than 650 farads. Hence the impulse upon which the transmission of the signal depends must have been propagated along the conductor by some other means than by charging its successive parts electrically; *i. e.*, fully, and in the ordinary sense of this expression. The 30 farads, more or less, which could have been generated before the signal arrived at the distant extremity of the cables, would have been consumed in charging the first six or seven hundredths of the conductor.

During my stay in Valencia, messages were effectively and distinctly transmitted in each direction by the use of an electromotor formed by a small percussion-cap containing moistened sand, upon which rested a particle of zinc. The current here evolved could scarcely have amounted to more than six or seven farads, so that nearly two minutes would have been requisite for charging one cable; yet the transmission-time was certainly small, although it was not definitely measured.

The experiments without earth-connection on November 6 and 9, differed from those of the 5th and 6th, only in that the Newfoundland battery consisted of ten cells instead of the same number as was employed at Valencia. The mean times of transmission were respectively 0ˢ.25 and 0ˢ.24, indicating an increase of speed with the increase of electromotive power. And, so far as the experiments on these

four days are concerned, we might infer that on the complete metallic circuit formed by the two cables, the time for transmitting the signals through about 3475 kilometers, or 2160 statute miles, was not far from 0ˢ.29 for a battery of 3 cells, 0ˢ.26 for one of 4 cells, and 0ˢ.215 for one of 10 cells.

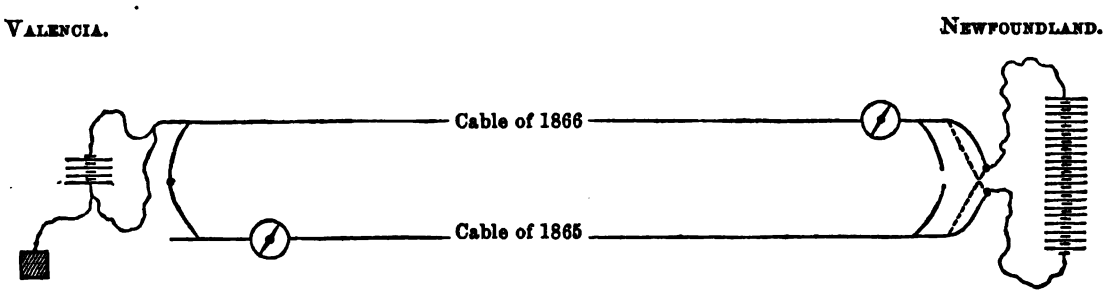
On the other hand, the average transmission-time for signals sent by a current induced in a single cable, by means of a "condenser" with a battery of 10 cells, was 0ˢ.31 on the 25th, and 0ˢ.34 on the 28th October; the mean interval for these two days being 0ˢ.328. Each of the condensers used possessed an electrostatic capacity of about 20 farads; so that with a tension of 10 cells, or 8.4 volts, their capacity would be not far from 168 farads, or equal to that of about 590 miles [945 kilometers] of cable—in other words, a little more than one-quarter of the capacity of one whole cable.

The value of those experiments in which the batteries were connected with the earth is seriously impaired by the series of mistakes made at Newfoundland on the 10th November. On that day 20 cells were used instead of 4, and the prescribed connection of the battery with the ground was forgotten, so that both the electromotive and the electrostatic relations became too complicated for any safe inferences as to the results. But apart from these, some other grave error appears to have been committed, by which we are apparently led to the singular result that the average time consumed in the transmission of signals was 0ˢ.31 for the positive, and only 0ˢ.24 for the negative signals; although the only difference between these classes consisted in an interchange of electrodes relatively to the two cables, and although the transmission-time for the two cables is shown by all our other experiments to be practically equal. The sole reason which I can discover for any difference between these two kinds of signals seems inadequate to explain the phenomenon, yet it ought not to be overlooked. It is this:—

The construction of the signal-keys was such that, in the only manner in which it was safe to use them for these experiments, the battery-circuits remained connected with the cables at the receiving station. The cables were connected with each other without the battery, and the battery was short-circuited independently of them; still, a metallic connection did exist between the telegraphic circuit which was formed by the two cables together with their transatlantic battery, on the one hand, and the temporarily disused (and also closed) local circuit, on the other. So long as there is no earth-connection in this local circuit, its effect may fairly be left out of all consideration; but whenever any such connection is introduced, the case is changed.

In the experiments of Nov. 10, the zinc of the 4-cell battery at Valencia was provided with an earth-connection, while the 20-cell battery at Newfoundland was insulated. And, since the galvanometer at each terminus was situated upon that cable to which the platinode was applied for those signals which we term positive, some difference must have existed in the action of the two classes of signals from Newfoundland upon the Valencia galvanometer. For the Newfoundland signals would exert a tension on the cable of 1866, which on reaching Valencia might act for an instant inductively upon the local circuit, before the dynamic equilibrium of the main circuit should be established by means of the opposite tension upon the

other cable, and the signal thus exhibited upon the Valencia galvanometer. Obviously, when the ground-connection was made with the zinc of the Valencia battery, this disturbing action would be the greatest for those signals of which the tension would thus be for a moment partially neutralized; namely, for the positive signals.



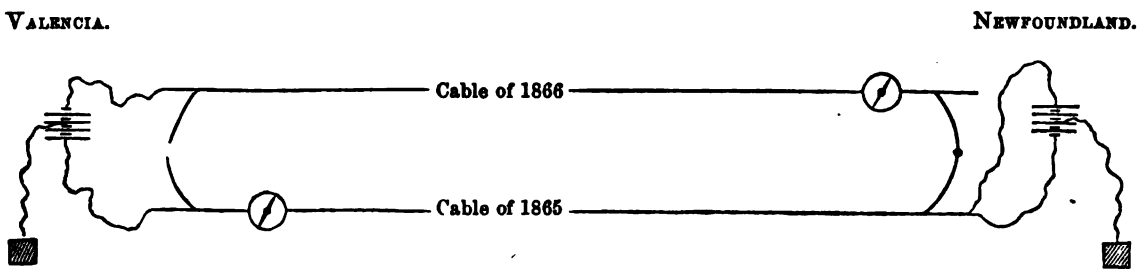
No other explanation than this has suggested itself; and though, as already stated, this scarcely appears adequate, it would yet derive some color from the absence of any analogous differences for the two classes of signals in the first experiment of the same day, in which the ground-connection was made to the middle of each battery. On the other hand, a similar though inferior difference does exhibit itself in the third experiment, where no earth-contact was made; and it seems safer to assume some additional and yet undiscovered mistake in the arrangement of the connections, and therefore to discard the observations of November 10 altogether, than to attempt to draw any inferences from them. These would contradict the experiments upon other days, when the connections were managed somewhat more effectively, although not without mistakes in the Newfoundland batteries on both the 6th and 9th of November.

On November 16th all the arrangements seem to have been correctly made, each battery consisting of 4 cells, and the earth-connections at both stations being made with the zincodes in the second experiment, and with the middle of the batteries in the third.

In the former case, all the positive signals found earth at the other extremity of their respective cables without affecting the second cable at all, and therefore without manifesting themselves upon the galvanometer at the distant station; while negative signals, which differed from the positive ones only by the interchange of the cables used for the respective electrodes, were of course received and recorded. Thus we have on this occasion only the "negative" signals; *i. e.*, those in which the platinodes went to the cable of 1866 at Valencia, and to that of 1865 at Newfoundland.

In the latter case the effect of the arrangement would be to substitute two circuits (each consisting of one cable with two cells at each extremity and earth-connections), for the one circuit, formed by a cable with four cells at the signal-giving station and with earth-connections; were it not that a very small portion of each of the two first-named circuits is common to the two, being formed by the piece of metal which unites the short-circuit of the local battery with the connected or "looped" cables. This will be readily seen from the diagram, which represents a positive signal from Valencia. Both sets of signals from Valencia were received at New-

foundland; but, contrary to my expectations, only two of the ten positive signals from Newfoundland were perceptible upon the Valencia galvanometer, and these were but weakly indicated, the needle being much agitated.



The results of the recorded signals give 0ˢ.26 as the transmission-time through one cable with earth-return, when the ground-connection was made with the zinc, and 0ˢ.27 when it was made with the middle of the battery; the former corresponding to the use of four cells, at one station only, and the latter to two cells at each station.

Passing next to the consideration of the velocity of signals given by closing and interrupting the circuit, which for convenience we will call "make-circuit" and "break-circuit" signals, we have some data for the investigation from the first and third series of experiments. For the first series the battery was at Valencia, and the signals from Newfoundland were necessarily given by making and breaking the circuit for the battery at the other station, or, in the language of telegraphers, sending against the current. For the third series, the inverse was the case, and the Valencia signals were sent by means of the current from Newfoundland. In both instances the signals from the battery-station were given in the usual way by the alternation of opposite currents. That such an arrangement was ill adapted for any important electrical investigation is palpable; but such few experiments as were made were of course entirely subordinate to the object of our expedition, and were, as will be seen from the programme, very roughly indicated in advance. The totally different character of the methods and appliances from those which had come within the previous experience of our longitude-parties, as well as the very different nomenclature, rendered telegraphic instructions difficult, ambiguous, and, as the event proved, often ineffective. The circumstances under which our few simple trials were made were embarrassing, in spite of the cordial interest and friendly aid of the telegraphic staffs on both sides of the ocean. The cables were in continual requisition for commercial purposes, although all facilities were accorded which I could conscientiously ask. It nevertheless appeared desirable to make such few essays at measuring the time of transmission as opportunity conveniently allowed, in the hope that something of interest might prove deducible. And it will be perceived that our own experience could not be rendered available at the time, inasmuch as all inferences must be derived from the measurement and collation of chronographic records, which could only be brought into juxtaposition by some 3000 miles of transportation.

Our data, thus obtained, for the relative velocity of the make-circuit and break-

circuit signals, lead to the singular inference that the latter travelled most rapidly in the case of Newfoundland signals with the Valencia battery, while for the Valencia signals with the Newfoundland battery precisely the reverse was the case. For this I have no explanation to suggest. It has been impossible for me to shake off a suspicion that the same error in the connections on November 10, which occasioned the discordances heretofore mentioned, may have also acted to produce the discrepancies here manifested; but I will confine myself to a statement of the results, and leave any possible reconciliation of discrepancies for the future.

There are two ways in which the comparative velocity of these two sorts of signals may be examined. One is by comparing the values of the approximate longitude, as given by the make-circuits and break-circuits respectively, for which purpose all corrections for clock-error, &c. may be disregarded, since they affect both sets of signals alike. The other is by deducing the sum of the transmission-times for each kind of signals taken together with the signals sent in the opposite direction. This latter method permits the employment of a much larger number of observations, and by use of the value of the transmission-time for the positive and negative signals, as previously deduced, it allows a tolerably approximate determination of the actual time for the signals in question. The former gives only the difference between the intervals consumed by the two classes respectively, but it affords measures of this difference free from the influence of extraneous sources of error. I will state the results obtained by each of these methods.

Beginning with the first named, it will readily be perceived that an excess in the approximate longitude, as deduced from make-circuit signals indicates an inferior velocity for these, when they are sent from Newfoundland eastward, but a superior velocity when they are sent from Valencia westward. Yet such an excess is manifested in both cases, as will be seen from the appended table.

SERIES I.—SIGNALS FROM NEWFOUNDLAND; BATTERY AT VALENCIA.

Exp't.	Date.	Earth-connection.	Make-circuit signals. No.	Make-circuit signals. Mean interval.	Break-circuit signals. No.	Break-circuit signals. Mean interval.	Excess for make-circuits.	No. of cells.
I. 3	Nov. 1	None	2	$2^h\ 52^m\ 15^s.515$	4	$2^h\ 52^m\ 15^s.335$	$+0^s.180$	20
I. 1	10	Middle	3	22.293	4	21.997	+0.296	4
I. 2	10	Zinc	9	22.293	10	22.040	+0.253	4
I. 3	10	None	10	22.091	10	21.965	+0.126	4

SERIES III.—SIGNALS FROM VALENCIA; BATTERY AT NEWFOUNDLAND.

III. 1	Nov. 16	Middle	9	$20^s.950$	10	$20^s.798$	$-0^s.152$	4
III. 2	16	Zinc	10	21.002	10	20.880	−0.122	4
III. 3	16	None	10	21.082	10	21.030	−0.152	4

The results by the second method of inquiry may be obtained by assuming the transmission-time for signals from Valencia, November 1, to have been $0^s.214$, and that for signals from Valencia, November 10, and from Newfoundland, November 16, to have been $0^s.264$; Thus we have :—

SERIES I.—NEWFOUNDLAND SIGNALS, VALENCIA BATTERY.

Exp't.	Date.	Earth-connection.	Transmission-time. Make-circuits.	Transmission-time. Break-circuit.	Excess for make-circuits.
I. 2	Nov. 1	Zinc	0s.65	0s 39	+0s.26
I. 3	" "	None	0.80	0.61	+0.19
I. 1	Nov. 10	Middle	0.54	0.22	+0.32
I. 2	" "	Zinc	0.72	0.30	+0.42
I. 3	" "	None	0.47	0.31	+0.16

SERIES III.—VALENCIA SIGNALS, NEWFOUNDLAND BATTERY.

III. 1	Nov. 16	Middle	0s.44	0s 64	—0s.20
III. 2	" "	Zinc	0 43	0.56	—0.13
III. 3	" "	None	0.36	0.34	+0.02

These values are rudely confirmatory of those deduced by the first method. They show at any rate a difference in velocity for the two kinds of signals, which becomes very large when the tension at any part of the circuit is disturbed by an earth-connection. And they also indicate that a full charge or discharge of the cable is not requisite for a make-circuit or break-circuit signal.

In the experience of the Coast Survey since 1851, the break-circuit signals, which have exclusively been employed for longitude-determinations, have varied comparatively little in their velocity. This question has been investigated in every instance; and, in many cases, large changes have been made in the battery-power and in the connections, for the purpose of observing the effect upon the transmission-time. I have no access to the records of these experiments at present; but the results have in general shown, that with a well insulated line of uncoated iron wire, of the size ordinarily employed[1] (the earth itself forming half the circuit), the time required for the signals to reach their destination is not far from 0s.07 for each thousand miles, or, roughly, that their velocity is 22,000 kilometers to the second. The necessary interpolation of repeaters between Heart's Content and Calais precludes any determination of the velocity of the electrical action; but the average interval of time consumed in the passage of a signal between these two stations was 0s.277, the distance being 1090 miles, and four repeaters being interposed.

During the intervals between the signals, the electrical condition of the cable was undisturbed, and no extraneous influence prevented its return to a state of equilibrium. The signals were a quarter of a second long, as nearly as might be, and intervals of five or of ten seconds elapsed between the successive signals, each pair of "sets" having fourteen intervals of 5s each, and five intervals of 10s. Upon no one of the five longitude-nights was there any direct connection between the cable and the earth. The two extremities of the cable were connected with condensers on the 25th and 28th October, and all signals on those occasions were therefore given by induction only; while on the 5th, 6th, and 9th November, a complete circuit was formed by the two cables, and the battery at the receiving-station was short-circuited. On these last two nights the two cables were not con-

[1] That called in commerce No. 9, weighing about 320 pounds to the mile, or 78.4 grams to the meter.

nected at the sending station during the intervals between the signals, but the battery was short-circuited there also. Thus the cables were always resuming their equilibrium between the signals, during each of the five nights when the exchanges for longitude were made; there being upon the first two nights only one length of cable used, but upon the last three a double length, through which the adjustment of the perturbation was to be effected.

I will give the results for these five nights in the same form in which they were first presented; viz., the mean difference between the records of the same signals upon the two registers, this being the resultant value of the longitude, uncorrected for clock-errors or for transmission-time. The 2h. 52m., which are common to all, can be here omitted, only the seconds and fractions of a second being needful for our purpose, and the signals are assorted according to the length of the interval which immediately preceded. On each date three series, of 20 signals each, were sent from each station, but not all were received. The average number upon which the several values for each day actually depend, is 16 positive and 22 negative, after the five-second intervals, and 6 positive and 5 negative, after the ten-second intervals.

UNCORRECTED VALUES OF LONGITUDE.

ASSORTED BY LENGTH OF INTERVAL PRECEDING THE SIGNAL.

Date and signal-station.	No. cells.	5ˢ interval. Pos.	5ˢ interval. Neg.	10ˢ interval. Pos.	10ˢ interval. Neg.	All. 5ˢ	All. 10ˢ
Oct. 25. Val.	10	9ˢ.075	9ˢ.060	9ˢ.033	8ˢ.975	9ˢ.068	0ˢ.092
Newf.	10	10.235	10.265	10.308	10.332	10.250	10.320
Oct. 28. Val.	10	10.406	10.388	10.376	10.445	10.397	10.401
Newf.	10	11.678	11.677	11.710	11.742	11.373	11.723
Nov. 5. Val.	3	17.313	17.287	17.272	17.243	17.299	17.261
Newf.	10	18.446	18.467	18.481	18.482	18.457	18.480
Nov. 6. Val.	4	17.214	17.220	17.224	17.185	17.217	17.211
Newf.	10	18.285	18.298	18.335	18.372	18.292	18.350
Nov. 9. Val.	4	20.288	20.281	20.251	20.267	20.284	20.257
Newf.	10	21.370	21.350	21.344	21.373	21.359	21.357

Hence we may infer the sum of the transmission-times in the two directions to have been

Date.	5ˢ	10ˢ	Excess for 10ˢ interval. Val.	Newf.	Mean.
Oct. 25	0ˢ.576	0ˢ.622	—0ˢ.024	+0ˢ.070	+0ˢ.046
28	0.670	0.716	—0.004	+0.050	+0.046
Nov. 5	0.552	0.613	+0.038	+0.023	+0.061
6	0.469	0.532	+0.006	+0.058	+0.064
9	0.469	0.493	+0.027	—0.003	+0.024

Taking next the results afforded by the experiments of November 10 and 16, we find the mean difference between the records of the same signal at the two stations (omitting the 2h 52m as before), to be:—

Exper't and signal station.	No. of cells.	Earth-connection.	5ˢ interval. Pos.	5ˢ interval. Neg.	10ˢ interval. Pos.	10ˢ interval. Neg.	All. 5ˢ	All. 10ˢ
I. 1. Val.	4	Middle	20ˢ.874	20ˢ.894	20ˢ.840	20ˢ.815	20ˢ.886	20ˢ.830
I. 2. "	4	Zinc	20.698	20.876	20.680	20.840	20.800	20.744
I. 3. "	4	None	20.790	20.792	20.677	20.780	20.791	20.718
II. 1. Val.	4	Middle	20.893	20.869	20.876	20.845	20.879	20.863
Newf.	20	None	20.898	21.921	21.903	21.890	21.911	21.898
II. 2. Val.	4	Zinc	20.692	20.870	20.690	20.780	20.792	20.735
Newf.	20	None	21.918	21.940	21.937	21.935	21.931	21.936
II. 3. Val.	4	"	20.758	20.827	20.760	20.800	20.798	20.780
Newf.	20	"	21.897	21.909	21.960	21.955	21.904	21.958
III. 1. Newf.	4	Middle	22.232	22.262	22.260	- - - -	22.251	- - - -
III. 2. "	4	Zinc	22.307	22.301	22.247	22.345	22.304	22.306
III. 3. "	4	None	22.290	22.270	22.263	22.300	22.279	22.278
IV. 1. Val.	4	Middle	21.192	21.150	21.157	21.215	21.166	21.180
Newf.	4	"	- - - -	22.316	- - - -	22.285	22.316	22.285
IV. 2. Val.	4	Zinc	- - - -	21.195	- - - -	21.180	- - - -	- - - -
Newf.	4	"	- - - -	22.315	- - - -	22.300	- - - -	- - - -
IV. 3. Val.	4	None	21.188	21.164	21.133	21.150	21.174	21.140
Newf.	4	"	22.270	22.259	22.317	22.295	22.271	22.308

whence we find the sum of the transmission-times, in the two directions in the experiments when batteries are used at each station, to have been

Exper't.	5ˢ	10ˢ	Excess for 10ˢ interval. Val.	Newf.	Sum.
II. 1	0ˢ.426	0ˢ.429	+0ˢ.016	—0ˢ.013	+0ˢ.003
II. 2	.533	.595	+0.057	+0.005	+0.062
II. 3	.500	.572	+0.018	+0.054	+0.072
IV. 1	.544	.499	—0.014	—0.031	—0.045
IV. 2	.544	.514	+0.015	—0.015	0.000
IV. 3	0.491	0.562	+0.034	+0.037	+0.071

The mistakes, heretofore mentioned, at Heart's Content in the number of cells and in the connections on the 10th November, put it out of our power to make any definite inferences from the first two experiments of Series II; and the number of signals after intervals of 10ˢ, in the first two experiments of Series IV, was so small as to forbid much reliance upon their mean. But the evidence here also indicates that a longer time was consumed in the transmission of signals after the longer interval.

Finally, the first and third series of experiments (in which a battery was employed at one station only) give the following results for the relative speed of the make-circuit and break-circuit signals, four cells being used in every instance.

NOV. 10. SIGNALS FROM NEWFOUNDLAND; BATTERY AT VALENCIA.

Exper't.	Earth-connection.	5ˢ interval. Makes.	5ˢ interval. Breaks.	10ˢ interval. Makes.	10ˢ interval. Breaks.	Excess of time for make-circuits. 5ˢ	10ˢ	Diff.
I. 1 . .	Middle	22ˢ.293	22ˢ.000	- - - -	- - - -	+0ˢ 293	- - - -	- - - -
I. 2 . .	Zinc	22.260	22.056	22ˢ.360	21ˢ.975	+0.204	+0ˢ.385	+0ˢ.181
I. 3 . .	None	22.107	21.979	22.063	21.910	+0.128	+0.163	+0.025
NOV. 16. SIGNALS FROM VALENCIA; BATTERY AT NEWFOUNDLAND.								
III. 1 . .	Middle	20.920	20.796	21.010	20.935	—0.124	—0.075	+0.049
III. 2 . .	Zinc	21.045	20.876	20.977	20.895	—0.169	—0.082	+0.087
III. 3 . .	None	21.067	21.030	21.120	21.030	—0.037	—0.090	+0.053

It is thus manifest that in general a longer time was required for the transmission of signals after an interval of ten seconds, than after an interval of five seconds. In those cases where no earth-connection existed, and the signals were alternately positive and negative, the cable was meanwhile assuming its electrical equilibrium, so that a positive signal was transmitted more rapidly through the conductor when it was affected with a larger amount of negative electricity, and a negative signal more rapidly through a conductor containing more positive electricity. This affords new testimony to the erroneous character of the supposition that the conductor must be charged through any portion of its length, in order to transmit a signal beyond this portion.

As showing the continued existence of currents (doubtless engaged in establishing equilibrium) during the intervals between the signals, it may be of interest to mention that on one occasion when the two cables had been joined at Heart's Content without battery, and while the Valencia battery had been temporarily disconnected, signals from Newfoundland were distinctly received. They were weak, and the deflections of the needle were scarcely one-fifth as large as usual, yet they were none the less distinct, and a complete set of signals, ten in number, at proper intervals and preceded by a "rattle," was recognized at Valencia. No other record of them was made, than the fact of their transmission by alternation of the make-circuit and break-circuit signals, although no battery had been connected with the cable for several minutes.

On the 16th of November I made a series of experiments at Valencia, for the purpose of ascertaining the effect of changes in the electromotive force upon the speed of the signals, and whether these signals could, by the interpolation of any resistance between them and the galvanometer, be made to traverse the double length of the cable before reaching the galvanometer at the same station.

The results of these experiments may be very briefly stated, after mentioning some details regarding the signal-key or commutator. The construction of this key was such, that very little time was lost in pressing down either button, the interval being as nearly as I could estimate, about one-seventieth of a second, or approximately 0ˢ.015. All signals by which currents were sent were given in this way, but the break-circuit signals were given by removing the thumb from the button, which was

then lifted by the tension of the spring. This tension being less than the muscular force of the thumb when the button was pressed down, a longer time was consumed in traversing the distance between the stops; and, for this, repeated measurements give 0ˢ.035 as a near approximation to the average interval. Now since, as already related, the ordinary signals record themselves upon the chronograph when the arm carrying the button leaves one stop, but are not really given until it reaches the other, all the recorded intervals between the instants of giving and receiving make-circuit signals will be too large by about 0ˢ.015; while for break-circuit signals the reverse obtains, and the recorded interval will be too small by about 0ˢ.035. Consequently, in comparisons between break-circuit signals and others, a correction must be applied, varying with the temporary adjustment of the signal-key, but amounting on the average at Valencia to not far from 0ˢ.05. The importance of this correction will be recognized on inspection of the results of the first four experiments of the following series. It has, nevertheless, not been applied to any of our results, inasmuch as during the exchanges between Valencia and Newfoundland, no measurements or estimates were made to determine this pass-time for the Newfoundland key. It must, of course, be taken into account in any attempts to draw inferences regarding the relative velocity of break-circuit signals.

The signals in these experiments were given by Mr. Mosman, and recorded by myself, using the circuit formed by the two cables without any other connections than the same key, galvanometer, and battery at Valencia, which had been employed for the other work of the expedition. Care was of course taken that the signals should be neither seen nor heard by myself, except as indicated by the deflections of the galvanometer-needle.

Exp. I. 4 cells. Circuit made and broken. Key between zincode and galvanometer.

	No.	Mean interval.
Make-circuits	11	0ˢ.257
Break-circuits	11	0.229

Exp. II. 4 cells. The same, with 126 ohms resistance between key and galvanometer.

Make-circuits	10	0.279
Break-circuits	9	0.227

Exp. III. 4 cells. Key and galvanometer upon opposite sides of the battery.

Make-circuits	13	0.278
Break-circuits	14	0.225

Exp. IV. 4 cells. The same, with 126 ohms resistance between key and cable.

Make-circuits	11	0.287
Break-circuits	11	0.220

Exp. V. 1 cell. Positive and negative signals.

Positive.		Negative.		Both.	
No.	Mean.	No.	Mean.	No.	Mean.
2	0ˢ.240	8	0ˢ.292	10	0ˢ.282

Here the moments for the positive signals were only recognized with difficulty, 8 out of 10 being lost. The battery-power was insufficient to move the needle promptly, with the existing adjustment of its damping-magnet. The difference in this respect between the two classes of signals was very marked, although they alternated at the prescribed intervals of 5 and 10 seconds.

			Positive.		Negative.		Both.
			No.	Mean.	No.	Mean.	No.	Mean.
Exp. VI.	2 cells.	Positive and negative signals.	10	0ˢ.249	9	0ˢ.242	19	0ˢ.246
Exp. VII.	4 cells.	The same.	8	0.268	10	0.290	18	0.279
Exp. VIII.	10 cells.	The same.	10	0.270	10	0.245	20	0.258
Exp. IX.	10 cells.	Resistance of 25 ohms interposed between key and galvanometer.	10	0.254	10	0.258	20	0 256
Exp. X.	10 cells.	Resistance increased to 251 ohms.	9	0.287	10	0.289	19	0.288
Exp. XI.	10 cells.	Resistance increased to 2513 ohms.	10	0.305	9	0.286	19	0.296
Exp. XII.	10 cells.	Resistance increased to 25130 ohms.	11	0.288	10	0.299	21	0.293

From these experiments it may fairly be concluded:—

1. That there was no real difference in the interval for the make-circuit and the break-circuit signals. The mean from the first four experiments gives, after application of the corrections for pass-time of the key, an interval 0ˢ.261 for the make-circuits, and 0ˢ.260 for the break-circuits.

2. That the relative positions of key, galvanometer, and battery exerted no perceptible influence upon the result, when a battery of 4 cells was employed. The mean intervals from the first two, and from the second two experiments, are 0ˢ.258 and 0ˢ.262 respectively.

3. That no appreciable effect was produced by the interpolation of 126 ohms' resistance. The mean intervals with and without this resistance, were 0ˢ.258 and 0ˢ.263.

4. That no marked diminution of the interval was produced by an increase of the battery from 2 to 10 cells. The results with 1 cell, although untrustworthy, indicate a somewhat less interval. The others vary by less than their probable errors, yet the interval was certainly not greater with 2 cells than with 10.

5. From the last three experiments it would appear that the interval was slightly longer after resistances above 250 ohms had been introduced. Yet it was no longer in the 12th experiment, when the resistance between the key and the galvanometer was more than two-thirds greater than the whole resistance of the two joined cables, than in the 11th when it was only one-sixth as great as that of the two cables.

6. We have every reason for believing that in all these twelve experiments, the measures of the intervals were merely determinations of my own personal equation in noting signals, which, as has been shown in Chapter IX, had been found by special investigation to be about 0ˢ.275. The variations from this value amount in but few cases to more than ± 0ˢ.03, which we have seen to be the normal range.

7. These experiments are entirely confirmatory of what would have been anticipated from theory, viz., that a signal given by closing a galvanic circuit is transmitted in both directions simultaneously, and with equal velocity under similar

circumstances; so that under no ordinarily practicable circumstances could a signal from either station fail to traverse both parts of the circuit at that station before passing on to the other.

Since the investigation[1] in 1850 to which I have alluded, the progress of science has thrown light upon many points which then were subjects of doubt or of individual opinion. The condition of an open galvanic circuit is now almost universally conceded not to be essentially different from that of an interrupted conductor to an electrical machine. The velocity of a current is also known to be dependent upon its quantity, and therefore generally upon its intensity, as well as upon the resistance of the conductor. But it appears questionable whether the law is so simple as has been supposed by some, who have regarded the velocity as inversely proportional to the capacity of the conductor multiplied by its resistance, and therefore, in a homogeneous conductor, to the square of its length. For the problem, as it now presents itself, does not pertain so much to the time for transmission of a given signal, as to the time for its transmission with a certain force, depending on the sensitiveness of the receiving apparatus; since the electrical impulse or disturbance consists of a continuous series of molecular influences which propagate themselves in every possible direction according to the inverse ratio of their several resistances. And the form of the conductor, as well as other conditions, may essentially modify the time requisite for the attainment of the prescribed force at the other extremity of the line. A current may thus be temporarily established in part of an open circuit, continuing until the battery and conductors have attained an electrostatic equilibrium. The time required for attaining this equilibrium depends of course simply on the capacity and form of the conductors, and on the energy of the battery; but the first electrical impulse may reach the most remote point of the circuit before a portion nearest the battery has received its full charge. Similarly, in a closed circuit, the distant extremity of the line may well be supposed to perceive some slight electrical disturbance from a signal, before its full force is manifested at intermediate points; so that a signal might be received with a delicate galvanometer at the farther extremity, before it could be recognized upon an electromagnet at half the distance. And this, too, apart from any consideration of increasing intensity in the electromotor.

The circuit formed by the two cables might, although broken at Valencia, thus serve to establish what would practically be a momentary current at Newfoundland when the battery at that station was introduced, deflecting the galvanometer there for an instant; and the change of statical condition in the cables at Valencia would thereupon be manifest to the electroscope. But the closure of circuit at Valencia would be accompanied by instantaneous deflection of the galvanometer, with corresponding insensibility of the electroscope. Thus a signal given by closing or interrupting an insulated circuit at any point is instantaneously transmitted from that point in both directions, and at full speed; but the interval before it attains its total force at any other point, must depend upon the character of the intervening conductor.

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[1] Proc. Amer. Assoc. Adv. Sci., 1850, p. 71; Am. Jour. Sci. XI, 67, 154.

The question as to the route by which signals are transmitted, when part of the circuit is formed by the earth, is thus disposed of; and the position maintained in the memoir above cited seems entirely corroborated, although it loses its theoretical significance. Prof. Kuhn, in his learned and valuable *Handbuch der Elektricitätslehre*,[1] while doing the fullest justice to the former investigation in other respects, takes exception to the propriety of my inferences regarding this question, but careful reconsideration has failed to convince me of any flaw in the argument, such as it is, notwithstanding my distrust of any reasoning from which so eminent a physicist would dissent.

Our experiments with the cables are inadequate for any decided deductions regarding the relative velocity when the earth forms a part of the circuit, but it may be well to examine for a moment what they appear to indicate.

The transmission-time for the several signals in our exchanges of November 10 and 16 may be approximately determined by a method different from those which we have thus far employed. Since the experiments occupied but a comparatively short time on each of these days, we may suppose the clock-errors to have remained constant during each series. Then, from those experiments in which no earth-connection was made, we may deduce the constant difference of the two clock-times; and a comparison of this quantity with the difference of clock-times as deducible from any set of signals will afford a near approximation to the actual time of their transmission.

Thus we have from II. 3 and IV. 3, supposing the speed the same in each direction—

Date.	Signals.	Diff. of records.	Error of noting.	True interval.	Diff. of clocks.
November 10.	Valencia,	$2^h\ 52^m\ 20^s.790$	$+0^s.331$	$21^s.121$	$2^h\ 52^m\ 21^s.382$
	Newfoundland,	21.917	—0.275	21.642	
November 16.	Valencia,	21.184	+0.331	21.515	2 52 21.753
	Newfoundland,	22.236	—0.275	21.991	

and adopting these values of the difference of clocks, we obtain as the transmission-times—

Experiment.	Signals.	Pos. & neg.	Make-circuit.	Break-circuit.
I. 1.	Valencia,	$0^s.202$		
	Newfoundland,	- - - -	$0^s.633$	$0^s.343$
I. 2.	Valencia	0.271		
	Newfoundland,	- - - -	0.636	0.383
I. 3.	Valencia,	0.301		
	Newfoundland,	- - - -	0.434	0.308
II. 1.	Valencia,	0.181		
	Newfoundland,	0.308		
II. 2.	Valencia,	0.279		
	Newfoundland,	0.288		
II. 3.	Valencia,	0.260		
	Newfoundland,			

[1] Allgemeine Encyklopädie der Physik. Bd. XX, p. 494, Leipzig, 1866.

III. 1.	Valencia,	- - - -	0.472	0.624
	Newfoundland,	0.267		
III. 2.	Valencia,	- - - -	0.420	0.592
	Newfoundland,	0.284		
III. 3.	Valencia,	- - - -	0.340	0.392
	Newfoundland,	0.252		
IV. 1.	Valencia,	0.264		
	Newfoundland,	0.332		
IV. 2.	Valencia,	0.224		
	Newfoundland,	0.262		
IV. 3.	Valencia, Newfoundland,	0.238		

The experiments IV. 2 and IV. 3 differ only in that the return-circuit is formed by the earth in the former case, and by the second cable in the latter. The transmission-time appears in both instances to be 0ˢ.24. For the Newfoundland signals in Experiments II. 2, and II. 3, the same difference exists, and the transmission-time appears to be 0ˢ.28 in the former, and 0ˢ.26 in the latter case. It would seem therefore that the velocity was but little, if any, affected by this great change in the character of the circuit, with a battery of 4 cells.

In the first and third series, the signals from one station were given by breaking and making circuit, but from the other in the ordinary way by alternate currents, so that the 2d and 3d experiments of each series differed from one another by the tension of the zincodes having been destroyed in the former by an earth-connection, leaving the tension to reach the cables from the platinodes only. The results give

	Val. I.	Newf. III.	Mean.
Experiment 2,	0ˢ.271	0ˢ.284	0ˢ.278
Experiment 3,	0.301	0.252	0.276

or an average transmission-time of 0ˢ.28 in each case, using 4 cells.

In the first experiment of Series I and III, one half the circuit was formed by the earth, while the cables had 2 cells at each end. In the second experiment of Series IV, the earth formed one half the circuit, and the cables had 4 cells at the sending station. The results give:—

	Val.	Newf.	Mean.
I. 1, III. 1,	0ˢ.202	0ˢ.267	0ˢ.234
IV. 2,	0.224	0.262	0.243

The Valencia signals of Series I were made November 10; all the others were on November 16, without other difference of circumstances than those in the connections as described. No difference in the velocity appears to have been produced by the changed arrangement of the 4 cells which constituted the battery.

It is not without hesitation that I present the facts and inferences of this chapter. For I am not unaware of the careful and thorough quantitative investigations of Thomson, Jenkin, and others, and should of course shrink from publishing these relatively crude and very incomplete results, were it to be supposed that I regarded them as comparable with those obtained by those distinguished electricians. But

the opportunity of adding some few facts to those heretofore established seemed worth improving, although obtained with no special apparatus, and entirely collateral and subordinate to the astronomical purposes of the expedition. And furthermore, the question has an especial interest for me, as having been among the first to demonstrate and measure nearly twenty years ago the transmission-time of the galvanic signals, which had previously been assumed to be instantaneous. The duration of our signal-currents was intended to be uniformly one-quarter of a second, but depended upon the skill and care of the observer, no automatic signal-giver having been employed. Every electrician knows how greatly the strength of the current is augmented by an increase of its duration from $0^s.2$ to $0^s.3$; yet the duration of the signals varied frequently through a larger range than this. Still the actual length of each signal is recorded upon the chronograph-register, and its average did not vary much from the prescribed duration of $0^s.25$.

It appears manifest that not an electrical charge or discharge, but simply an electrical disturbance, is requisite for transmitting a signal; that an inductive impulse, sufficient to deflect the galvanometers employed, was transmitted through one cable, having at each end a condenser with 10 cells, in somewhat less than the third of a second, five seconds after the transmission of an impulse of the opposite sort; that with a circuit formed by the two cables, a smaller electromotive force sufficed to transmit the signals with yet greater rapidity; that the signals travelled more rapidly through a cable which had not recovered its electrical equilibrium after a current of the opposite character; and that the speed of the signals is modified by the earth-connections, more readily than by changes[1] in the battery-power. And the very marked differences, found in the rates of transmission, between signals given by completing an interrupted circuit and those given by interrupting a closed circuit, may perhaps lead to investigations which will afford an explanation.

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[1] Jenkin (Phil. Trans. CLII, 982) arrived at the conclusion that the electromotive force of the battery has no appreciable effect on the velocity with which the current is transmitted. But he would doubtless consider that some qualifications to the general statement should be taken for granted.

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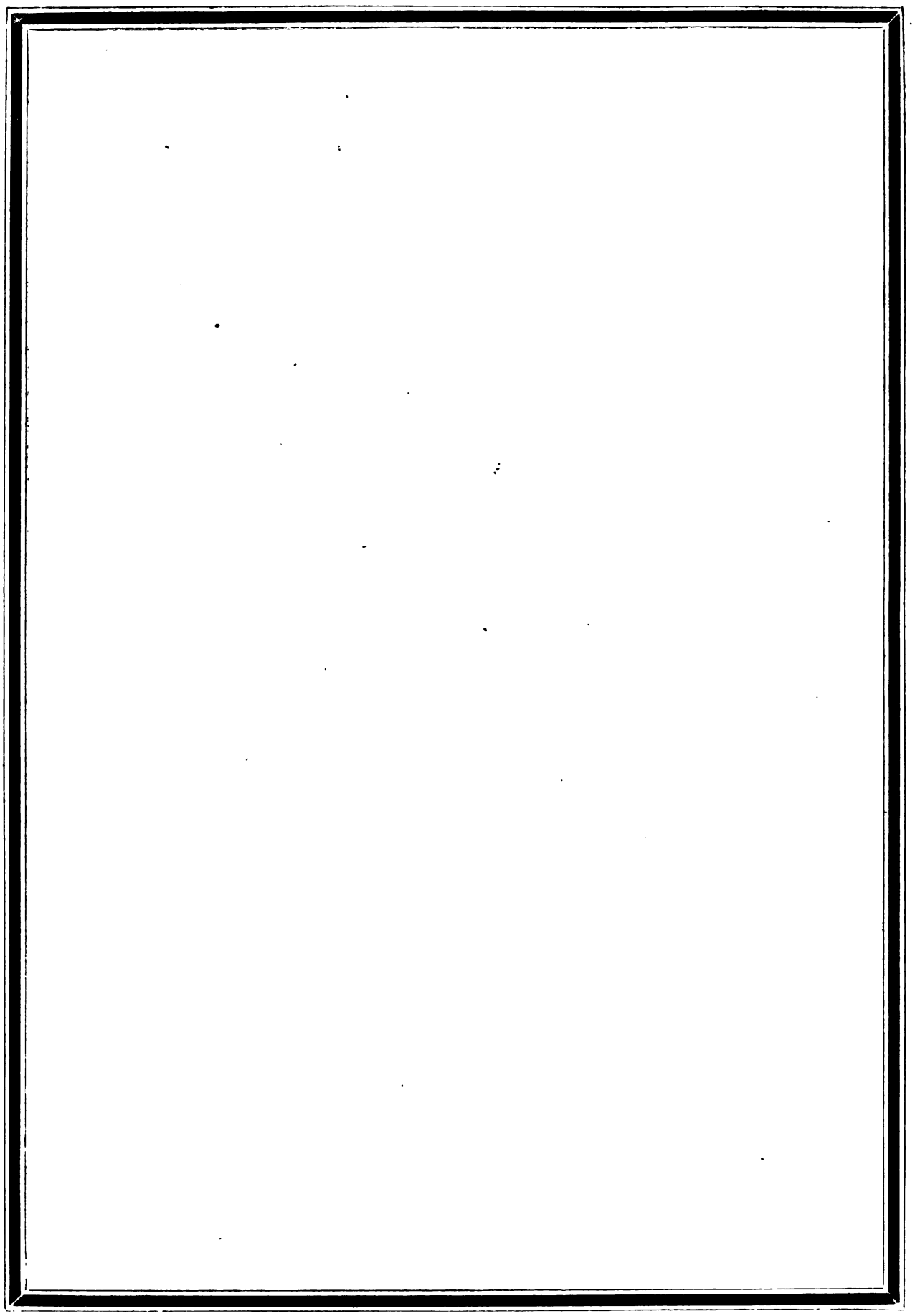

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